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THE QUARTERLY REVIEW
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WHY ARISTOTLE INVENTED THE WORD ENTELECHEIA (*Continued*)

BY WM. E. RITTER

SECOND FOREWORD

LETTERS evoked by the portion of this article published a year ago, have shown me that nowhere in the substance of the article do I make clear the ground of my interest in the problem discussed. That ground is, first and most deeply, my interest in Nature—in all Nature—not in a few parts of it that happen to strike my personal likes or dislikes. This interest grew, slowly and long, sometimes interruptedly but never menacingly, and led me, finally, to "accept the Universe."

Though always hesitant about being too hard on the Universe, even on the parts of it that were least to my taste, it was not till after my period of infallible knowledge about it and cantankerous criticism of disapproved parts of it (period, i.e., of early and middle life) that I became duly impressed with the serene way Nature gets along in the face of fault-finding with her. Not till then did my acceptance become whole-hearted.

But other factors contributed to the acceptance. One of the most influential of these was the clarification of my understanding that we ourselves are parts of the Universe in so vital a fashion as to be privileged, indeed compelled, by the very nature of the case, to contribute to making the Universe what it is on any particular day of our lives.

Seeing that by no hook or crook can we do otherwise, it is best, then, for us to accept the Universe—*as a reality*. But, seeing further that we cannot avoid contributing to changes in that reality, it is best for us to see to it as far as possible that these changes are good for us.

Vast scientific researches have made the interrelatedness of parts of nature far more manifest to us than it has been to previous generations. We ourselves and the myriad other living things, likewise the earth, the sun and all the heavenly hosts, without which we neither live nor move nor have any being—of such does reality turn out to be.

Travel on this highway led me in due time to recognize the need of some one word or phrase for expressing a truth about reality that is not expressed by any English word. Aristotle's famous word *entelecheia* comes very near filling this need. The phrase "complete reality," adopted by W. D. Ross as an English equivalent for the word, is justifiable so far as my very meager knowledge of the Greek language enables me to judge. "Fulfilment" Professor Ross kindly suggests now by letter as a possible single-word equivalent. This would be fine if it could include the idea of genesis as unmistakably as does "complete reality;" for the inclusion of this idea is unquestionably one of the chief merits of the word.

These paragraphs are a revision of the *Foreword* to the installment already published. Their purpose is to make clearer my primary interest in this study. But I want also to make clear my secondary interest, the question of Aristotle's attitude toward, and conception of, Nature. Intrinsically the man was cast in the same spiritual mold, according to my view, that all the great naturalists of all the ages have been cast in.

Of these naturalists I mention only two, restricting myself thus for a reason that will appear as we go along. The two chosen are Charles Darwin and T. H. Huxley.

In several late publications it is pointed out that all naturalists may be ranged in two great subdivisions. For one of these the terms *naïve* and *analytic*, for the

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other *philosophic* and *synthetic*, have been suggested. Neither the naming nor the defining of the groups is particularly easy. There is, however, one characteristic that is of almost unfailing diagnostic value. That is the presence or absence in a given naturalist of interest in, and effort on, the nature of knowledge itself, the *epistemology* of traditional philosophy.

Any naturalist who "accepts the universe" in the sense above indicated, i.e., not only because he can't help himself but because he is eager and glad to, thereby commits himself to some serious attention to the problem of knowledge. This for the obvious reason that having accepted himself as part of the universe, he has accepted all of his activities. Among these activities are his getting and using of knowledge. So it turns out that every such naturalist belongs willy-nilly, to both the subclasses of the class of naturalists we are recognizing.

But so well-nigh universal is the custom among modern students of the natural sciences to limit their studies to the external world that any tendency otherwise has tended to bring ill repute upon the venturer. To be "objective" is the unfortunate term chosen as descriptive of the proper method of procedure. The need for more critical attention to the meaning of this and many other words used by naïve naturalists is one of the things that makes it important for every naïve naturalist to become also a philosophical naturalist in some measure. (In anticipation of the legitimate query as to what my pretensions to being a philosophical naturalist rest on, I refer to a few, rather fragmentary, writings of this purport which have thus far been published: Ritter and Bailey, 1927, 1928, 1929; Ritter, 1929, 1931.)

So prodigious in size and complexity of structure and action is the portion of nature outside of us, and so fascinating is it, too, that it is neither surprising nor deplorable that the vast majority of naturalists should occupy themselves very largely with the study of this portion of nature. But it is also neither surprising nor deplorable that a considerable number of naturalists should be more interested in the processes themselves of getting and using knowledge than in the things known and used.

Of all those who have accepted nature the most fully and devoted themselves most single-mindedly and effectively to the study of her in the fashion we have denominated as naïve, no one seems to have equalled, much less to have surpassed, Charles Darwin. The implication of this, that the exalted place held by Dawrin among scientists is due to his labors as a naïve naturalist to the almost entire exclusion of labors as a philosophical naturalist, is likely to come with something of a shock to most, perhaps to all, scientists. Yet in so far as occupation with the problem of knowledge is taken as a criterion of philosophy, Darwin's aloofness can be shown with great clearness. All professional philosophers who have given attention to the point seem agreed on this. The truth of it deserves more attention than it has received.

T. H. Huxley, on the other hand, is allied with the philosophical as well as with the naïve naturalists. His *Hume, Hume with Helps to the Study of Berkeley*, and portions of his articles on *Agnosticism* led him far into the problem of knowledge. These writings mark him as a philosophical naturalist quite as definitely as his writings on marine invertebrates, on fossil vertebrates, and on human anatomy and physiology mark him as a naïve naturalist.

In unpublished manuscript of the larger work of which this article is a part, I draw quite a "parallel lives" of Aristotle and Huxley. In the likeness in type of the two, nothing stands out more sharply than their common characteristic of *naïve-ness* on the one hand, and *philosophic-ness* on the other hand. Both did extensive researches on marine invertebrate zoology, that is to say, on animals far down the zoological scale in one direction; and also on man and the higher vertebrates in the other direction.

The conclusion I reach about Aristotle may be epitomized as follows: In original responsiveness there is every reason to suppose his bent was toward accepting nature in the sense indicated. His childhood and early youth, lived in a home environment of medicine and surgery and a neighborhood environment of hills, valleys and streams, and of sea and shore, as these entered so largely into the physical geography of the almost primitive Macedonia of that time, would be especially favorable for such a bent. A flood of particulars—sensory particulars—largely primal, never-ceasing, ever-varying, poured upon the boy from all sides. Research of late years has convinced us that the influence of environment on the mentality of young children is such that conditions like those under which Aristotle lived as a boy are by no means insignificant for the rest of life.

Then came the transplantation of the youth to the vastly different environment, that of a city and a culture of peerless grandeur. Athens and the Academy!

The flood that poured upon the youth and young man for almost twenty years—what a contrast it was from the flood that had poured upon the boy! Universals now, not particulars, were its chief substance. And this meant thoughts more than sense perceptions. Subtleties of expression, dialectic and rhetoric more than names, descriptions and classifications of observed objects were now the constituents of the flood.

But no environment whatever is the whole story for any living being. The being itself is an essential—

an exceedingly essential—part of the story. Particularly is this so if the being is as highly constituted as is a human being. Most particularly of all is it so if the human being happens to be an Aristotle.

Readily as the youth responded to, profoundly as he was influenced by, the stream that flowed from the Athenian and Academic environment, this stream could not entirely obliterate the influence of the flood to which he was subject as a boy. Consequently, as he neared the maturity of manhood and passed beyond the precincts of the Academy, he began to feel, we may conjecture, that the kind of thing he experienced years ago in Macedonia was not less real than the kind he had now experienced in Athens, and hence that the two must be in harmony at some great depth of truth.

Distressingly scant though the biographical evidence is, it justifies the surmise that at this critical period of his life Aristotle caught a gleam that led him to become, finally, the "type specimen" (this technical term I am entitled to use from my vocation as a taxonomist in natural history) of the subspecies philosophical naturalist, of the species naturalist, according to our classification.

The truth, a gleam of which I imagine him to have caught, concerns the phrase: "accept the universe." Is the phrase to be taken as merely these two words with some ill-defined meaning attached to them? Or is it to be taken with a meaning that corresponds to the objective truth about the Universe and the emotional-rational truth about accepting? The constitution of the universe is such that it must be accepted both in its state of wholeness and in its state of compositeness. He who attempts to accept it on any other basis than its *oneness* on the one hand, and its *manyness* on the other hand, makes the attempt at the peril of his own oneness, or integrity. As to size and number of parts, the universe has no limits so far as observational knowledge has been able to ascertain. As to the creative and sustentative powers of the universe, there are no observably demonstrable limits to its powers of sustaining already existing parts and of producing new parts.

Of these truths about the universe, Aristotle caught hardly more than a gleam. How could he have done better? So small a portion of the universe, and so few of its parts were accessible in his day, the wonder is that he should have caught even this much.

But catching even this much of the truth sufficed to endue him with a sensitiveness to all the things round about him, with curiosity about their deepest meaning, and with determination to satisfy himself on these scores. Accordingly, when about forty years old he laid out for himself the task, recognizable by us now as inconceivably vast, of acquiring a knowledge and understanding that should be coextensive in size and commensurate in detail with the Universe.

He would produce a science of the whole Universe in contradistinction to the science taught at the Academy which he seemingly (and I think rightly) regarded as in effect an attempt to avoid accepting exceedingly important parts of the Universe. The historic fact and the probable significance thereof that Aristotle turned with greatest seriousness to the study of nature *after* his extensive studies in the Academy, appear to be gaining recognition by specialists in the Aristotelian writings. All naturalists measurably acquainted with the whole situation would, I think, agree that such pretended natural knowledge as that presented in the *Timaeus*, for example, could be no more acceptable to Aristotle as a naïve naturalist than the Platonic Forms and Ideas were acceptable to him as a philosophical naturalist.

But the undertaking that Aristotle proposed for himself was gigantic even in his day, relatively small and simple though the Universe then seemed. As a naïve naturalist he must describe, name, and classify all its lands and waters and the inhabitants thereof; the sun, moon and stars; the blue sky; the air with its winds and storms; and whatever else might be contained in the flood of sensory experience. Furthermore the causes, potencies, and actualities of all these, he would have to consider.

The attempt to carry out this part of his program was responsible for bringing upon his head the modern withering criticism of superficiality. From spreading himself over everything and consequently being thorough in nothing, his labors, though universally admitted to be almost unsurpassed in volume, are viewed by some exact scientists of our day as valueless if not worse. It is true that much of all natural science since the Greek period may be looked upon as additions to Aristotle's beginnings and corrections of his errors. This is particularly true for the sciences of inanimate nature. But there is much truth in it too for the sciences of animate nature—the sciences in which his chief researches were made.

But, foundational as were Aristotle's attributes as a naïve naturalist, more definitive of him were his attributes as a philosophical naturalist. For the problem of knowledge was his main concern, as everybody knows, in his "*First Philosophy*" (called *Metaphysics* by later students) and in his group of treatises generally called by his successors the *Organon*, or *Logic*. The final test of how far the man succeeded in carrying out his grand project must be sought here.

Far be it from me to presume myself competent to apply this test fully. My task is much smaller. It is only that of finding the use Aristotle made of his

term *entelecheia* in his treatment of the problem of knowledge, with a view to learning whether he could use it, when speaking as a philosophical naturalist, consistently with his use of it when speaking as a naïve naturalist. My efforts speak for themselves, chiefly in the portion of this article here presented. Since they contain my most important restrictions on, and deviations from the Aristotelian teachings I make this expiative remark: Allusion was made above to the patent fact that the vast progress of natural science since Aristotle's time has consisted largely in adding to and correcting, what he did. Adding and correcting thus is in the very essence of natural knowledge. Especially is this true of such knowledge as belongs to the realm cultivated by naïve naturalists. Why, then, should we not expect that something of these processes would operate in the realm cultivated by philosophical naturalists? But the nature of scientific and philosophic knowledge would justify the expectation that addition and correction would play far lesser parts here. Generalization, abstraction, ideation, reason, bringing great masses of sense data under a common viewpoint, are just what make philosophical naturalism as an indispensable correlate of naïve naturalism, the two together constituting Naturalism in its full meaning.

Whatever of merit or demerit may belong to Aristotle in mankind's effort to know itself and the world of which it is a part, this much, according to my interpretation, will stand to his credit as long as this desire and effort shall continue: He saw, as none before him and few since, have seen, that success in the Great Enterprize is contingent upon the acceptance of the Universe both as *One* Whole and as *Many* parts taken one by one, to constitute the whole. Any interpretation of Aristotle that would class him as a sub-naturalistic atomist or elementalist or as a supra-naturalistic "holist," is untrue to the deepest nature of the man.

So far as the ground of this interpretation is reducible to a minimum of single words, those words are: Δύναμις (dunames), *power, potentiality*; Ἐνέργεια (energeia), *activity, actuality*: and Ἐντελέχεια (entelecheia) *complete reality, fulfilment*. Of these the last is far-and-away the most important since it takes account of not only the whole, any whole, but of all its constituent parts, and of its coming to fulfilment by a course of individual development.

A postscript to those, some of them friends or acquaintances, others personally unknown to me, who have made valuable comments and criticisms on the portion of the article already published: It is a great satisfaction to me, and will, I hope, be reassuring to readers generally, that in every case these commentaries (some of them from highly competent technical sources) agree that the main point of my contention is well taken.

For reasons that seem obvious it is impracticable to take advantage now of all the useful suggestions given me. But when and if the article comes to publication in the whole of which it was written as a part, these and such further suggestions as may come, I certainly count on for improvements of that whole.

II

CAUSE AND THE WHOLE-AND-ITS-PARTS

In the first installment of this essay we carried the examination of the concept of a whole-and-its-parts, as worked out to a large extent by Aristotle, far enough to see something of what is involved when the concept is applied to individual human beings. But very much more remains to be seen in this direction.

The "complete reality" (*entelecheia*) of such a being certainly includes the entire gamut of its conscious (as well as infra-conscious) experiences.

Now, nothing is more certain than that within these experiences are the most exalted aspects of rational and spiritual life on the one hand, and the most unqualifiedly physical aspects of nutritional and sexual feeling and action on the other hand.

What, from this standpoint, did Aristotle do for these aspects of human life? Two negative but yet important answers are readily given: (1) We may be sure he did not overlook or minimize the reality and importance of either aspect. We know him to have been too good as both naturalist and philosopher for that; (2) we may be sure he could not possibly, however capably he might try, be fully successful as to either aspect. His knowledge of the structure and actions of animals and of natural bodies generally was much too scanty and error-pervaded. Consequently, in striving to answer our question, we must follow the course pursued thus far in this study. We must examine what Aristotle

knew and *thought* and *did* in the light of what moderns *know* and *think* and *do*.

The examination may well begin with a more careful scrutinizing and appraisement than we have yet made of the factual data now in our possession in their bearing on the problem of the causal relations between a body in its wholeness and the parts of which it is composed. Readers of the Aristotelian writings hardly need reminding further of the relation of this problem to Aristotle's "our usual method of investigation," the method, namely, of analyzing a whole into its parts. But sufficient notice has not yet been taken of the relation between this problem and the Aristotelian ideas of a whole-and-its-parts and of form and matter. With nearly all of modern science the notion has much the force of a religious dogma that to analyze a compound into its least parts is to explain that compound causally. Or at any rate, so the dogma holds, if in a particular case the explanation is not quite ultimate, this is because the analysis has not yet gone far enough. The ultimate goal of explanation is, *per se*, where analysis ends. So far as the inner nature of compounds is concerned, causality works in one direction only, i.e., from parts and elements toward the whole. Indeed, so far as concerns the nature of compounds, it is hardly too much to say that cause is conceived to be that which the parts, especially the least parts, do to and for the compounds they compose.

Such a notion as that a body in its wholeness may act causally on its parts not only has almost no place in modern science, but with the ultra-analytic school such a notion is heresy and should be treated as such. Cause must be so conceived and defined as to exclude the notion.

Something of the scope and depth of the Aristotelian disagreement with this position is known to everyone acquainted with the Aristotelian writings.

The involvement of the idea of cause in these writings is of course one of the characteristics and one of the specially hard and controversial things about them.

A brief examination of this question appears the more opportune at present because of what General Smuts has given us (or rather, has not given us) on the question. No one can, I think, read *Holism and Evolution* and other writings of this author on this general subject without being deeply impressed. But for me, at least, the impression is considerably mixed owing to the fact that I find no evidence, either direct or indirect, that the author has studied seriously, if at all, the works of Aristotle in their bearings on the problem of wholeness (Smuts, 1925, 1931).

Modern science, biology especially, has taken much cognizance of some of the toughest of these things. A general discussion of the controversial matters here is not to be thought of, nor would it be more than slightly relevant to our main purpose. There are, however, three things in the Aristotelian teachings about *cause* that are sharply relevant to our purpose. Two of these, the material cause and the formal cause, have been discussed almost endlessly in the past. The third, the principle of multiple cause, appears to have received little if any close attention. Yet the evidence that Aristotle recognized this principle to some extent is conclusive and seems more significant for estimating the relation of his knowledge and thought to modern knowledge and thought than has been appreciated. One piece of evidence on the point is this:—Following the definition of the four causes in the "Philosophical Lexicon" we read: "These, then, are the causes, and this is the number of their kinds, but the varieties of causes are many in number. . . . Causes are spoken of in many senses" (*Metaphysics* 1013^{b} 28–30 Ross trans.) Although many illustrations, by inference at

least, of like purport could be given, the statement itself will have to suffice.

We turn now to what I interpret as really a modernization of one aspect of Aristotle's formal cause. That aspect, expressed in terms consonant with the growing demands of theoretical biology, is that to the extent (which is certainly large) that Aristotle's idea of form is the lineal forerunner of modern morphology, his formal cause is the lineal forerunner of the idea that a living compound whole is as truly causal of the organism as are the parts or elements of which it is composed. I venture to express, dogmatically at first, what it seems to me the modern position is or soon must be, on this question: ***Every whole, especially every living whole, acts causally on its parts as well as being acted on causally by its parts.***

The interaction of the parts of any body to make that body alive is now so far understood that it is doubtful if the most extreme particleist in any of the sciences of living nature would defend the proposition that any of the parts, even the most minute and supposedly potent, produce, just *per se*, the aliveness of an organism. True, one often hears that the atoms of modern physics—or perhaps more exactly the electrons and protons, one or both,—must be alive. But those who express this view admit, of course, that this atomic life is quite different from the obvious life of any particular plant or animal. For nobody would seriously argue that an oak tree's aliveness and a man's aliveness are exactly the same, even though the electrons and protons of the two are the same.

So it seems fair to say that there is now general agreement among competent students that living wholes are dependent on the interaction among their parts. The reciprocal action of the parts—their reaction to one another—their causal action, each upon each—are productive of results in the form of bodies and actions different from the parts. There is no longer any factual or critically logical ground for holding that because a bacterium or an amoeba or a jelly fish or a dog or a man is alive, therefore the atoms of carbon, nitrogen, oxygen and the rest are alive and were alive before ever they combined to make these organisms. The indubitable facts compel the conclusion that the potentialities of the atoms are such that when they interact among themselves and combine to make large compound bodies, if the interaction and combination occur under proper conditions, the potentialities of the atoms become actualities as the characters or qualities or attributes of the particular organisms. Even vitalists, especially if working biologists, would probably go this far in recognizing the potency of the parts of organisms.

Now this modernly requisite conception of potentiality and actuality is in full accord with the general tenor of the Aristotelian teaching.

Crucial for the interpretation of the phenomena is the question of the "proper condition" for actualizing the potentialities as indicated. And here the Aristotelian theory, resting largely on logical grounds, is distinctly superior to modern theory, which also rests largely on logical grounds.

With the superiority of the Aristotelian over the modern logic in the case we shall not here concern ourselves since we have touched its ganglionic center so to speak, under the heading "The Law of Identity." "Law of Contradiction" would probably have been more appropriate for the discussion here referred to—if the laws of "formal logic" must be noticed at all in such discussions. But the concrete facts are, of course, the point: A human individual in the baby stage of his development *is* in that stage and is *not* in the

adult stage. Otherwise stated the characteristics of the adult are present in the baby *only potentially*. Much shoddy thinking about, and worse dealing with, children seems to result largely from disregard of these psychobiological facts. Our main point now is concerned with fact rather than with logic. For we are going to point to facts with which the Aristotelian concept of formal cause (as defined above) is more easily harmonizable than the concept of elemental-atomic cause of present-day biological orthodoxy.

NUTRITION, METABOLISM, AND THE WHOLE-AND-ITS-PARTS

The chemistry of assimilation and metabolism in living bodies is now too well understood to permit any informed person to repeat, except as a reminiscence or a joke, the old epigram, "One is what he eats."

The facts-in-common between the basic processes in an organism and in a flame are an explanatory commonplace of our day.

Consequently, just as no ordinary mortal can be fooled into believing that the stick of wood alongside his fireplace, or the surrounding air are "essentially the same" as the flame and the warm room that wood and air will cause if they come together in the right way—even as little can such a mortal be fooled into believing that he or any of his friends are "essentially the same" as the food they eat, the air they breathe, and the water they drink.

In connection with this allusion to the similarity between respiration and combustion further reference should be made to the great part played by the concepts of potentiality and actuality in Aristotle's thinking and the seemingly insignificant part these play in the speculative thinking of some moderns. Of course, no "dirt farmer," or cook, or engineer, or practical chemist, or maker or user of explosives, or, in fact, anybody faced with almost any practical problem, gets on independently of the principles though he may not think much about them.

If, however, we attempt to push the organismal conception beyond the certainty of the interaction of parts to produce the living organism, we find ourselves in trouble. Especially are we troubled if we try to push the conception with reference to human organisms. However, we shall now see that if one singles out himself as the human organism on which to test the conception, he gets important help for the general problem.

I am fully convinced that the mental and physical activities involved in my thinking and writing these lines are caused by the way my physical parts utilize the air I breathe, the food I eat, and the water I drink. But I am absolutely certain no physiologist or physicist, or chemist, or any combined efforts of theirs can discover by analyzing me exactly how my parts act and interact with the substances I take, to produce these effects. My certainty is thus positive for the reason that by the time my analyzers reach the crucial stage of their laboratory undertaking, *I*, the living organism now sitting at my desk, thinking and writing, will be dead and gone for several days at least. In other words, my analyzers will be trying to do one or the other (or both) of two impossible things: (1) Trying to observe causal processes *in actuality* which went on and were ended, many days or weeks ago; or (2) trying to discover by deductive reasoning parts and substances that possess the potentialities for accomplishments of man which the analyzers know, but only from the records. They learn nothing about these accomplishments *from their analyses*. The things they are trying to do we are sure are impossible first and foremost from examining critically our own knowledge-

getting processes. But our assurance is greatly strengthened by learning that this impossibility was recognized more than two thousand years ago by Aristotle and has been recognized time and again since by competent students.

Accordingly, being certain that, however far laboratory study of the parts and substances of me may go in explaining my thinking, and my acting generally, it is bound to fail at the most crucial point, no argument, just as such—argument, that is, that may profess to transcend entirely all sense data—could convince me that *I*, myself *whole*, am as truly a *vera causa* of all I am and do, as are any of the parts of me or the substances which enter into my composition. After admitting, fully and gladly, all that analysis can prove that my parts and external substances contribute to making me what I certainly am, I am not fully explained until I my very self am recognized as also causal of me.

At least to the extent that my thoughts and acts and conscious experiences differ from those of any other person, I see no causal explanation of the fact consistent with great masses of indubitable natural knowledge other than that *I* in my individual wholeness act through my parts to get from the air I breathe, the food I eat, and the water I drink, the energy necessary for producing the thoughts and acts and conscious experiences thus uniquely mine. It seems that *my assimilation and metabolism are as specifically mine as are any of my conscious acts or other conscious experiences.*

But as I look around I find great numbers of other individual organisms so like myself but also so differing from me and all other individuals, that I am obliged to suppose they, too, are uniquely privileged in their assimilation and metabolic powers.

Nor is this all. Extending my inventory of other individual organisms I notice not only endlessly slight (relatively) differences among great numbers of individuals, but also endless differences that are by no means slight, and that are endless in kind and grade. For instance my inventory contains not only white men, black men, red men and so on, but fishes and trees and amoebae. And then I recall that by almost endless labors men have discovered that, *mirabile dictu*, all these endless kinds and grades of individuals exist, grow, and act by taking from the world external to them the same few simple materials and using them to their own individual needs and ends, just as I do!

How could any botanist or chemist or philosopher explain, causally, an eucalyptus tree and an oak tree that have flourished for fifty years within a hundred yards of each other, their roots buried in soil that any expert would pronounce essentially the same and their branches, leaves and blossoms bathed in the very same air and sunshine, without including the two trees themselves as causal factors —if any such factors are recognized?

The truth is, the thing cannot be done. Nor can it be seriously attempted without ignoring or perverting some of the most deep-rooted and wide-reaching principles of both common and technical knowledge. And such attempting is the sort of thing Aristotle characterized as the childishness of trying to explain the obvious by the unobvious.

This problem of the way living beings, men particularly, use materials from external nature is so important that we shall do well to look at it from still another angle.

Reflect on what Mayow, Priestley, Scheele, Lavoisier and whoever else had a hand in the business, did in discovering oxygen. Really they did not begin by discovering a gas, having this name. They began with certain observations, that is to say with certain sense experiences. As they

worked along, these experiences became to them sense data, which they recognized as the sensible qualities, or properties of a certain gaseous body. What Mayow called fire air (*igno-aerian*) and Priestley dephlogisticated air, represent this stage in the course of the discovery. Finally came Lavoisier who by adding certain quantitative observations to the (chiefly) qualitative observations of his predecessors, recognized the body as not really a kind of air, but so distinct as to deserve a name by itself. The word, oxygen, which he devised, was chosen, notice, because of what in his experience, was its most characteristic property, that of generating (in combination with other bodies) still other, compound, bodies that were "sharp" to the taste and in their action on many bodies. The newly discovered body was acid-producing.

Every step in this discovery as in every other that was ever made in any division of natural knowledge, involved basically sense experiences, accepted later as sense data, still later as sensible qualities or properties of external bodies, which qualities, or properties are peculiar to, or characteristic, or definitive of the bodies. In other words, the *observed*, or *sensible characteristics of any body are not only peculiar to that body, but are those things by which we* KNOW THE BODY.

And now comes a still more vital point. It is known to everyone that the history of the discovery of oxygen (Mayow to Lavoisier, about a hundred years ago), is involved with the basic similarity between respiration and combustion—between breathing and burning. So much a part of common knowledge is the dependence of the activities of living bodies, at least of all higher plants and animals, on oxygen, that the merest reference to it is enough for this discussion.

See then the conclusion that the discovery of oxygen seems to force upon us: *While Mayow and the others were discovering, by their powers of observation, the sensible properties of this gaseous body, they were also demonstrating by their powers of living that this same body possesses life-giving properties that are discoverable only by* LIVING AND PERFORMING EXACTLY THOSE ACTS THEY DID PERFORM IN MAKING THE DISCOVERING. (Ritter and Bailey, 1928, p. 250)

The incalculably vast number of potentialities of oxygen which this interpretation requires might stagger, even mystify, us except for the recognition that the potentialities of oxygen, like those of all other bodies, are really non-existent for us mortals except as actualized by the interaction of the bodies with other bodies. Were if not for the living that was done by Mayow and the others, the life-giving potentialities of oxygen would never have been actualized and discovered. Nor is this all we know that does much toward saving us from being hopelessly mystified by our conclusions. Not only are living men and other organisms necessary for actualizing some of the potentialities of the oxygen, but various familiar substances which constitute the food and drink of organisms are necessary. Carbon and nitrogen are particularly in evidence as such necessities. The truth is, the entire mass of knowledge possessed by us moderns is available as aids to making such a conception of oxygen (and all the other chemical elements upon which life depends) intelligible to us. It is *intelligible* to us because it is the reasoned counterpart of our *experience* as living organisms.

If we desire to think the problem or the basis of some theory of cause, we ought to find, it would seem, genuine satisfaction in the concept of multiple cause. For here many supporting facts from modern science are available, as well as the fact

that the idea is recognizable in the history of knowledge and thought as far back as Aristotle at least.

One may justifiably wonder, it seems to me, whether the bewildering discoveries being made by physics and chemistry in our day on atomic structure and action may not mean that sensory knowledge is not now touching the hem, so to speak, of the garment that has been and is the common experience of all living things.

THE "WILL" AND THE WHOLE-AND-ITS-PARTS

From the light one may gain on the general problem of a whole-and-its-parts by studying himself as a chemical transformer so to speak, and then by asserting his "natural rights" as such a transformer, I am encouraged to take a still farther step in the same direction.

The aspect of myself to which I now turn is that of my wanting things, resolving to try for them, and finally actually carrying out the resolution as far as possible.

All this is so obviously at bottom an affair of my very own that no one would, I presume, venture to interfere with me much. If, however, I set up the contention that the "I" is myself whole, is I, *Body-and-Soul, Mind-and-matter,* all an inseparable *One,* I am pretty sure to be reminded that the interpretation almost universally accepted in such cases is very different from that I suggest. True, it would probably be granted, that my wanting and the rest are undoubtedly mine, "But," my correctors are quite sure to remind me, "it is your will and its adjuncts that really wants, resolves, and executes. Of course this is largely dependent on your body parts, especially when the executing stage is reached. But so well-nigh universal has been, and still is, the idea that human experiences of the sort you mention are the expression of something or other not closely dependent, at least not dependent in a fashion that is analytically demonstrable, on your body parts and substances, it seems the view must have a solid basis of truth and so should be retained. Furthermore, since the term, Will, appears to be as good a name as any for this analytically undemonstrable something, that, too, may as well be retained with its original meaning."

So here we are, quite innocent of intent or even of preawareness, face to face with the interminable problem of the will. But since the path that has led us to it is clearly marked though not much traveled, we can not honorably avoid facing the problem squarely. And here again I must intrude my personal experiences.

Being a hopeless addict to the habit of reading in bed at night when by well established laws of health I should be asleep, I suddenly find myself wanting an orange. Knowing that there is a bag of this fruit in the adjoining room, I decide that as soon as I have finished the chapter I am now reading ("Reflex Action and Theism" in Wm. James' *The Will to Believe*) I will go and gratify my want (want of being nourished and of having certain other felt needs satisfied). The end of the chapter having been reached, I close the book, lay it aside, throw back the bed covers, get out of bed, go to the orange-containing room, reach into the bag, bring out an orange, carry it to a table, open a drawer which I know contains the tools convenient for preparing the fruit to be eaten, and so complete the preparation for gratifying (satisfying?) the wants now upon me.

Then I go back to bed and think about what I have done.

Two points connected with my doings seem deserving of special attention. I notice that had I supplemented all the pronouns referring to myself—"I," "me," "my"—by such nouns as mind, memory, thought, desire, satisfaction, I should not thereby have increased by one iota my un-

derstanding, my achieving, or my sense of benefiting. If I say I have a desire or feel a need for an orange or anything else, I surely mean no more than if I say I desire or need the thing. Whenever I speak of a thought or a conception as "entering my mind" I am only saying in a round-about way that I think or that I conceive, so and so. The purposes in making our activities into abstract nouns seem various and are often far from obvious. There is one purpose, however, that is clear enough, in general: It is a convenient way of shunting into the background the puzzling question of exactly why and how one acts as he does. When I can say "my choice is made," I have a feeling of unequivocalness, of finality, about what I have done, that is more satisfying than if I say "I have chosen." In the latter case I feel somewhat more inclined, perhaps impelled, to explain, to analyze, what I have done.

Since these sentences were written it is with very great satisfaction that I have read the just-published book (Korzybski, 1933) in which the whole subject of the use of generalized and abstract words is given the most thoroughgoing treatment it has ever received, so far as I know.

Notice with me now what I do when I decide upon a particular course of action and then really act in accordance with that decision. When I decide in conformity with my wanting an orange, to go, at a specified future time, and do the things I believe will satisfy my wants, the question of what and how many parts of me are involved in making the decision, is surely a very complex, and at some points, obscure question. Thanks, however, to the splendid researches of modern psychologists, physiologists, histologists, and biochemists, I have, or may have, much of the answer to the question. But not an item in all this knowledge will have any meaning for the special case in hand, without *me*, a living organism, as the field of operation.

But it is when I put the decision-making aspect of me to the test of my executing aspects that many of my most concrete, most conspicuous parts, prove their rôle in the case. Throwing back bed covers, getting out of bed, walking to the next room, and all the rest—the body members involved no five-year-old will hesitate to name. Nor will anybody of down-right intellectual honesty refuse to grant my contention that they—hands and feet, for instance—are truly mine and that I truly do move them. Furthermore, some of my less familiar, though no less certain internal members no one would think of ignoring or of questioning my ownership of—nor the fact of my using them. But when the question is raised of what, exactly, all these internal parts are and how many there are of them, some of the difficulties in the way of getting direct observational answers are undoubtedly very great. My chromosomes and genes, for instance,—not to speak of my atoms, electrons and protons,—how am I to learn their part in the business? Accordingly the expert analysts might proceed, typically and legitimately, to give hypothetical answers, answers, that is (if really legitimate) intended to be provisional only. And "provisional" would have the two-fold reference of making the answer that would seem most likely to be true, and of aiding further effort toward the real, i.e., the observationally based, answer.

Notice now what my own attitude and view must be in all this. So far as my experiences, whether conscious or unconscious, were concerned, the observational distinctness or indistinctness of the parts involved, is not of the slightest moment. My desire for an orange, my decision to go after one, and my actual going, getting, and eating it, might have been as truly portions of my conscious experience when I was a five-year-old as when I was a seventy-

five-year-old. Such objective knowledge of my parts and their activities involved as I have gained by hook or crook, has not cut the least figure in the experiences themselves. Nor does it matter a scintilla, so far as my conscious experience is concerned, that the parts of me by which I want an orange defy the analyst's efforts at complete discovery. Such parts serve me just as certainly and readily as do such crass parts as hands, feet, and mouth.

Well, I ask myself, have I real ground for supposing that any desiring, deciding, resolving, and executing I may do in any realm or at any level, are teetotally different from my doings in the realm of orange eating?

True enough, my wantings, decidings, resolvings, and executings, involved in, for example, the part I took in producing the Scripps Institution of Oceanography, were remarkably different from those in the citrus example. But, really, so far as I can see, the difference concerned goal and operative details rather than basic facts. My act, or state, of wanting, just *per se*, appears to have been the same in the two cases. To want an institution for the purpose of the satisfactions that knowing the Pacific Ocean and its living inhabitants might bring—wherein does the wanting *qua* wanting differ from wanting an orange for the purpose of the satisfactions that eating it might bring? As right now I ransack my memory in connection with the two series of experiences, it seems to me literally true that the half-hour series connected with the orange was in essence a fair epitome of the two-decade series connected with the Institution. That the decisions, the executive efforts, and the satisfactions and dissatisfactions differed quantitatively immeasurably in the two series is too obvious to need asserting. As to qualitative difference—well probably, there was some, especially in the matter of satisfactions. There is, it seems to me, a difference between a physical satisfaction (nutritional for instance) and a psychical satisfaction (discovery of truth for instance), that is fundamentally qualitative. Perhaps the difference here involves the very essence of quality as does the difference between the different senses—sight and sound—for example.

But really what seems to me the deepest difference in the two series of experiences concerns satisfaction as contrasted with dissatisfaction. If my orange case had had in it (as it did not have) the element of obstacles to be met and entire or partial defeats to endure, as, of course, the Institution series had, the epitome of the latter by the former would have been more nearly exact. For instance had my wanting involved the getting of a whole meal instead of an orange; and had the getting of the meal depended largely on my own decisions and executive efforts, with my serious culinary limitations, the epitome would have been all that could reasonably be expected of an epitome. Nor would the case have been fundamentally different, so far as I can see, had what I wanted been a fortune, a woman's hand in marriage, the production of the greatest scientific discovery ever made, the greatest poem ever written, or the greatest social or governmental reform ever needed.

It will never do to let the views here expressed appear as though wholly isolable from certain other views. Of these "other views," particularly to be mentioned is that as to how wanting, the first member of the series, comes to be; and that as to how the actual acts, the last members of the series, accomplish the satisfaction at which the whole series aims. But while it would not do to leave this matter untouched, thus leaving the reader unaware (so far as this text is concerned) of its existence, the presentation of these "other views" would involve problems with which the present discussion is not concerned. It would be out of place here. And we must not permit anything to divert our attention from the main task in hand—that of discovering, if possible, how far the

interpretation that seemingly must be given *entelecheia* is applicable to man.

On the basis of a lifetime of conscious experiences of which the two here noticed are samples, and of the superposition upon these of much objective knowledge, I seem justified in defining what most authorities would call my Will as follows: ***The word, "Will," is a highly generalized noun or name for what I, a living whole, cause my parts to do toward securing what I believe will be good for me.***

Such a definition is likely to strike many persons as too shockingly absurd to merit a minute's attention. Nevertheless, I submit it with confidence that after a while it will be recognized as meriting much attention. One thing that contributes to my confidence is purely historical. A glance at the index of any good work on the history of philosophy suffices to discover that a theory, or doctrine of some sort, of the will has been near the center of the chief systems of philosophy from Augustine to Nietzsche and Bergson.

Now I submit that no one half awake to the modern spirit of science and philosophy could presume for a minute that a theory of the will which did not involve a sound theory of mental life could be even approximately true or moderately useful.

Accordingly the treatment of the will by psychologists since psychology has claimed its independence of traditional philosophy and its alliance with the natural sciences seems highly significant.

To go into this subject as extensively as even I, a non-professional in either philosophy or psychology might go, would be unjustifiable. But a few specially relevant points can be easily indicated. On the evidence of the long chapter, "Will," in James' *The Principles of Psychology* we know that this psychologist had no misgivings (in theory at least) about the will's being a subdivision of the mind and thus a subject to be treated first and foremost by psychologists. This comes to particularly sharp expression in the essay, *Reflex Action and Theism*: "From its first dawn to its highest actual attainment, we find that the cognitive faculty, where it appears to exist at all, appears but as one element in an organic mental whole, and as a minister to higher mental powers—the powers of the will." (James, 1897, p. 140) This from James, the professional psychologist, when upon occasion he turned philosopher. Rather curiously, when upon occasion Royce, the professional philosopher, turned psychologist, he saw Will in a different light: "The word 'Will' is of little use," we read, "as a purely psychological term, in the classification of mental life." (Royce, 1908, p. 334)

The main point of this, for us, is that, "Will" as Royce sees it, refers "*to the whole significance of our conscious life*" (italics original). This was one philosopher's way of saying that our decisions and actions are what they are because of our relation to our environment, or the world.

These views seem distinctly anticipatory of such other up-to-date views as that "Will is not precisely a psychological term, anyway, but is a term of common speech which need not refer to any psychological unit. (Woodworth, 1921, p. 523)

What looks to me like a more positive trend toward the conception of will I have formulated is the inclusion of it in a set of terms for "aspects of the total activity of the organism." (Gates, 1925, p. 463) And indicative of psychology's still further advance in this direction is the fact that some of the most up-to-the-minute schools appear to make no use whatever of the term "will." I venture to suggest that the farthest point yet reached by psychology on the road to such a conception of the activities of a living whole-and-its-parts as I am proposing, is the "behavior *qua* molar," set forth particularly by Tolman (1932).

It seems improbable that "molar" used thus would refer without important qualification to an individual animal (a man for instance) acting as a living corporeal whole. The molarity appears to connote mass as activity merely rather than mass as activity together with the acting body and all its parts. Nevertheless the conception comes so much nearer the true nature of living animals than does the conception of "behavior *qua* molecular" with which it is contrasted, that it may well be hailed by natural history as a great forward step for "pure psychology" to take.

We return for a moment now to the quotation from Gates about the activity of the organism. Shift the word "total" and make it modify "organism" instead of "activity" and the phrase would be readily interpretable in conformity with my definition of "will." For "total organism" would really have to mean, from the meaning of the whole phrase, what "living whole" means in my definition. Indeed there is little doubt that "total activity" of Gates's phrase implies the same thing. And in either case there would be implied, almost certainly, Aristotle's conception of *energeia* as applied to living beings, the actual organism acting as a whole. But it should be noted that the concept of *entelecheia* seems not to be implied.

Ridiculously brief is the sketch presented above of a human individual acting in two

particular cases, one at a rather low, the other at a rather high, level of his normal conscious existence. But I venture to hope the sketch may go far toward clarifying the conception that in both these cases or any other conceivable real case the individual, *whole*, that is to say, the individual consisting of all its parts acting in complete unification, was the main Factor, or Cause, or Principle—name it to suit yourself.

As a precaution against misunderstanding here I remark that the conclusion stated is not at all concerned with questions of relative excellence or greatness, or the reverse of these, of individuals. Rather, the concern is with the real nature of any and every typical human individual—with the question of what any and every typical human individual *really is*.

PROCREATION, SEXUAL PLEASURE, AND THE WHOLE-AND-ITS-PARTS

We pass now to a very different aspect of the central aim of this essay. That aim is, we never forget, the question of the relation of Aristotle's conception of "complete reality" (*entelecheia*) to the conception of a whole-and-its-parts as applied particularly to the human individual, the conception being viewed in the light of modern knowledge.

Mindful as we are of Aristotle's great respect for, and attention to, the phenomena of genesis, our previous notice of a favorite refrain of his about the begetting of men by men is convenient for connecting what we are now to present with what we presented earlier. It would be useless to try to understand Aristotle's attitude (as distinguished from his knowledge) relative to the problem of a man as a "complete reality" without considering his attitude (again as distinguished from his knowledge) *qua* man as a potential begetter of children.

In passing to this aspect of the subject we come upon defects in Aristotle's general system that are serious and by no means all pardonable, so far as I can see, on the ground of meager factual knowledge. The defects are in considerable measure due either to deliberate neglect or to faulty reasoning. Hicks' reference to this is brief and explicit. We read: Aristotle "exalted the cognitive element, while his treatment of the emotions and the will is wholly inadequate, even if the *Ethics* and the *Rhetoric* be called in to redress the balance" (Hicks, 1907, Introduction, p. lxxii).

We focus on the defects that are particularly relevant to this discussion. Aristotle was not, of course, blind either as a man or as a scientist and philosopher to the emotions and passions of sex. We know this about him as a man from the biographic record of his marriage, his devotion to his wife, and his being a father. And we know he gave real attention to the subject as scientist and philosopher from various things in his writings. Take this example:

"Thus there is one single moving cause, the appetitive faculty. For, had there been two, intelligence [*nous*] and appetency, which moved to action, still they would have done so in virtue of some character common to both. But, as a matter of fact, intellect is not found to cause motion apart from the appetency. For rational wish is appetency: and, when anyone is moved in accordance with reason, he is also moved according to rational wish. But appetency may move a man in opposition to reason, for concupiscence is a species of appetency." (*De Anima* 433^b 2–5, Hicks trans.). Although the work, *epithymia*, here rendered concupiscence, does not mean sexual lust exclusively, this meaning along with others seems undoubtedly to have been part of Aristotle's meaning. Such discussions as that on continence and incontinence in Book VII of the *Nicomachean Ethics* leave no room for question on the point.

Yet curiously enough in the elaborate and on the whole movingly admirable discussions of "Love or Friendship" and of "Pleasure" in the *Ethics*, we find scarcely a hint of the amorous element in the relation between a man and a woman! It is hard to see that there was any ground for Aristotle's formal teachings about women (about their being impotent men, being

classifiable with children and slaves and so on) other than the prevalent teachings of his age and country.

Really were it not for what the man gives us when he speaks as an unsophisticated zoologist, we might suppose he was too much of a Puritan or highbrow or something to discuss or even think much on the most domineering aspect of sex life.

After this was written I was glad to have my attention called to the fact that a few professional ethicists of today refuse to accept the traditional separation of reason and feeling. For example we read: "By enjoyment' we are meaning all the while a mental fact, conscious appropriation." (Fite, 1925, p. 212)

Fortunately, some of the things Aristotle says in the zoological works are sufficiently specific and are significant for our problem since they touch upon the climax of the sexual emotion. As would be expected, the most important of these are in *The Generation of Animals*. In his devastating criticisms of the theory attributed to Hippocrates that the semen comes from all parts of the body, the first of the four arguments in support of this Aristotle says is: "First, the intensity of the pleasure of coition; for the same state of feeling is more pleasant if multiplied, and that which affects all the parts is multiplied as compared with that which affects only one or a few." (*De Gen.* 721^{b} 15, Platt trans.). What specially concerns us is the explanation offered of the pleasure of copulation. Aristotle's reasoning against the theory proposed as to the source of the semen, important as it is, concerns us only indirectly now. That aspect of the problem we noticed sufficiently in the first part of this essay.

Obvious, is it not, that the explanation proposed of the extent of the pleasure would tally very well with the conception we are defending of the whole-and-its-parts? What, if anything, then, we naturally ask, has Aristotle to say touching this aspect of the problem? Unfortunately what he says is brief and only inferential. Yet the inference seems unescapable. "As to the vehemence of the pleasure in sexual intercourse," we read, "it is not because the semen comes from all the body, but because there is a strong friction (wherefore if this intercourse is often repeated the pleasure is diminished in the persons concerned)." (*De Gen.* 723^{b} 35)

Clear enough, then, is the recognition that the pleasure needs a causal explanation of some sort. So here it is: "And as to the pleasure which accompanies coition, it is due to emision not only of semen but also of a spiritus, the coming together of which precedes the emission." (*De Gen.* 728^{a} 10)

There you have it! *Pneuma*, that which imagination may make of the air essential to life, of the Zephyrs that stir the leaves, and of the tempests that lash great bodies of water into fury. What an amazing convenience for sophisticated thinking in the absence of factual knowledge!

Our only interest (except historic) in it is as to how the human organism would have to be involved to make the theory of sexual pleasure held by Aristotle consistent with his factual and theoretical knowledge as a whole.

The wording of the last quotation does not indicate that the author conceived the pleasure to be due to the involvement of the whole body with the spiritus. But even so when we have to do with such uncertainty as that of how *pneuma* transforms into spiritus, vagueness even to vagary of almost any sort, may be expected.

ARISTOTLE'S THEORY OF REASON AND OF PLEASURE IRRECONCILABLE WITH ENTELECHEIA

The question of how *pneuma* as he conceived it on the whole, we might examine at any length. But the futility of doing so, however well the task might be done,

is apparent once we remember that we are not much concerned with Aristotle's conceptions and teachings, *per se*. Our concern is with their validity as tested by modern knowledge. For such testing Aristotle has himself prepared the ground in this case.

His recognition that there is no sharp dividing line between reason and emotion we have already noticed. "Intellect," we have heard him say, "is not found to cause motion apart from appetency. For rational wish is appetency; and, when anyone is moved in accordance with reason, he is also moved according to rational wish. But appetency may move a man in opposition to reason, for concupiscence is a species of appetency." Put this from the *De Anima*, a biological treatise dealing primarily with mental phenomena, alongside the quotation given above from the *De Generatoine*, a treatise dealing primarily with reproductive phenomena, and notice what you have. The vital and familiar truth is presented in a somewhat round-about way that however exaltedly endowed with reason man may be he is yet driven at times with sexual desire.

So Aristotle almost forces us to ask whether the whole man as thus clearly indicated corresponds with his own more fully elaborated conception of man. Another, and for this discussion, more relevant way of asking the same question is: Is Aristotle's conception of man as unmistakably indicated in his zoological works (the *De Generatoine* and the *De Anima* particularly) the same, fundamentally, as that presented in his philosophical works (the *Ethics* and the *Metaphysics* particularly)? Finally, and still more to the point, we may ask the question thus: Is Aristotle's term *entelecheia* applicable to man as his conception of man stands in its most philosophical (metaphysical?) form?

Categorical answers to the last two forms of the question I believe must be: (1) Aristotle's conception of man contained in his traditionally philosophical works is irreconcilable in several ways with that contained in his traditionally biological works; (2) Not only is his concept of "Complete reality" (*entelecheia*) inapplicable to his philosophical (metaphysical) conception of man, but he himself probably felt this as he seems not to have applied the term at certain crucial points in his "first philosophy" of man.

A justification if possible of these answers is manifestly due from me The problem is necessarily involved in Aristotle's general theory of the psyche, or soul. So well known is his teaching concerning the separateness or at least separableness of reason, or the rational part of the soul, from the body that extensive treatment of the subject may be disposed of very briefly.

The following may be considered to present the kernel of his teaching on this great subject:

> Turning now to the part of the soul with which the soul knows and thinks (whether this is separable from others in definition only, or spatially as well) we have to inquire (1) what differentiates this part, and (2) how thinking can take place.
>
> If thinking is like perceiving, it must be either a process in which the soul is acted upon by what is capable of being thought, or a process different from but analogous to that. The thinking part of the soul must therefore be, while impassible, capable of receiving the form of an object; that is, must be potentially identical in character with its object without being the object. Mind must be related to what is thinkable, as sense is to what is sensible.
>
> Therefore, since everything is a possible object of thought, mind in order, as Anaxagoras says, to dominate, that is, to know, must be pure from all mixture; for the co-presence of what is alien to its nature is a hindrance and a block: it follows that it too, like the sensitive part, can have no nature of its own, other than that of having a certain capacity. Thus that in the soul which is called mind (by mind I mean that whereby the soul thinks and judges) is, before it thinks, not actually any real thing. For this reason it cannot reasonably be regarded as blended with the

body: if so, it would acquire some quality, e.g., warmth or cold, or even have an organ like the sensitive faculty: as it is, it has none. It was a good idea to call the soul "the place of forms," though (1) this description holds only for the intellective soul, and (2) even this is the forms only potentially, not actually (*De Anima* 429ª 10–28, Smith Trans.).

Probably no reader will need reminding that modern knowledge of the functions of the brain leaves the statement that thinking has no organ without a trace of fact to rest on. Nor will many readers fail to recognize in the statement about the soul as the "place of forms" one instance of Aristotle's seeming adherence to Plato's doctrine of Ideas.

Still more pointed about reason's independence is: "While the faculty of sensation is dependent upon the body, mind is separable from it." (429ᵇ 4)

As a revealer of what Aristotle knew and what he did not know about the psychobiology of man, and of his reasoning on the subject, these are crucial passages. The striking deficiency in his knowledge was of course anatomical and physiological: He knew nothing of the fact that brain is indispensable to thought. One might make considerable of the fact that in his comparison of perceiving with thinking Aristotle varies his language somewhat. While sensation is "dependent" on the body, mind is "separable" from it. Does this suggest that after all he had some misgivings about the complete lack of dependence of thinking on the body? May he not have surmised that as to its *origin* mind has some kind of dependence on the body while in its *functional* maturity it may not be thus dependent? Might not the mind's dependence on the body be something like an apple's dependence on the tree—dependent for origin but not for later existence and function? With all Aristotle knew about the generation and nutrition of animals and other living things it seems incredible that he should not have had some qualms about the separableness of reason from the body. So extensively and insistently does he present his conception of the nutritive and sensitive parts of the soul that I assume my readers sufficiently acquainted with this phase of the matter to obviate the need of going into it further. It will be enough, I hope, to assert and nothing more, that he never even suggests that these two parts of the soul are separable from the body. The rational, the thinking, part only be conceived to be separable. But how could he make this tally with what he knew and tells us so distinctly (as quoted above) about the way reason and appetency are tied up together?

But what specially concerns us is the bearing of Aristotle's belief in the separableness of mind from body on his conception of *entelecheia*. Can we get any light on this matter from studying the passages on separableness quoted above? They contain three points that are illuminating in this way.

The first is contained in the inquiry about the separableness of the soul. If we turn from the Smith translation (used in our quotation) to the translation used by Ross in his *Selections*, we find a difference that appears significant to a morphological biologist. The wording in the Ross version is: "With regard to the part of the soul by which it knows and thinks, whether this be separable or not separable in spatial magnitude but only in definition," etc. The point at issue is between the "spatially as well" of the separableness in the Smith translation and the "spatial magnitude" of the separableness in the Ross translation. *Megethos* is the term in the original which is rendered "spatially" in the Smith version, and "spatial magnitude" in the Ross version. Now since *megethos* means bulk or size, and hence implies shape, it is considerably more positive as to corporeality than is "spatially." This distinction is particularly significant because space has no objective reality for extreme idealistic philosophers, and appears to be in somewhat the same predicament at the hands of some present-day mathematical physicists. Viewed in this light the query is pertinent: Was not Aristotle virtually asking whether the thinking part of the soul *together with a part of the body* is separable from the rest of the body? This would seem in accord with his usual naturalistic attitude. So no wonder the man was puzzled—as we

know he was from passages like this: "Hence arises a question of the greatest difficulty, which we must strive to solve to the best of our ability and as far as possible. When and how and whence is a share in reason acquired by those animals that participate in this principle?" (*De. Gen.* 736^{b} 5).

The second point to be noticed touches the question from a different angle. Following the statement about the necessity for the mind's purity from admixture we read: ". . . . it [mind] too, like the sensitive part, can have no nature of its own, other than that of having a certain capacity."

That the thinking and the sensitive parts of the soul can "have no nature" of their own other than a certain capacity, or potentiality, is surely at odds with our modern knowledge of capacity, as we have seen in previous discussions. Nothing has just capacity alone. It is besides an entity in its own self. Explosives, for instance, and seeds, have natures of their own in addition to their capacities as explosives and as producers of full-fledged plants. So this makes it impossible to accept the statement as a valid argument.

But the weightier reason for calling attention to the point here is that it is not fully consistent with Aristotle's own general attitude toward nature, nor with some of his specific statements. Thus his repeatedly expressed view that a thing's nature consists in its matter, or substance, and its form, or final actualization, and also in the movements or changes by which it passes from one to the other, is entirely consistent with our present knowledge, but not with such a view as that here expressed. Take for instance his final, summed-up statement about nature in the chapter devoted to that subject in the lexical book of the *Metaphysics:*

> From what has been said, then, it is plain that nature in the primary and strict sense is the essence of things which have in themselves, as such, a source of movement; for the matter is called the nature because it is qualified to receive this, and processes of becoming and growing are called nature because they are movements proceeding from this. And nature in this sense is the source of the movement of natural objects, being present in them somehow, either potentially or actually. (1015^{a} 10–15, Ross trans.).

As a general statement this would seem to cover satisfactorily present-day information concerning the brain as an organ of thought.

The "somehow, either potentially or actually" of the fact that thought is present as a "natural object" in the brain would probably be satisfactory as a general statement to modern neurologists.

The third point which we notice involves directly the question about the bearing of the statements under examination on the problem of *entelecheia*. The essence of the point is in the statement (supposedly referring to Plato) about the soul's being "the place of forms." Directly following this, Aristotle adds his own limitation upon it: "though (1) this description holds only of the intellective soul, and (2) even this is the forms only potentially, not actually." The crux of the matter is that *entelecheia* is the term rendered "actually" in the translation. The original phrase is worth quoting: οὔτε ἐντελεχείᾳ ἀλλὰ δυνάμει τὰ εἴδη. "Not *entelecheia*, however, but dynamis is [the form or idea]." That is to say, the intellective soul being unmixed with body does not come under the concept of *entelecheia* because *entelecheia* includes both potentiality (*dynamis*) and actuality (*energeia*). In other words on the basis of what we are here given, it appears that Aristotle recognized that in conceiving the intellective part of the soul as separable from the body, he was placing it outside his concept of *entelecheia*.

Thus we have at the highest level of man's nature what is seemingly another piece of evidence that in Aristotle's real purpose *entelecheia* was fundamentally a descriptive term for his natural science and hence was excluded from the highest flights, so to speak, of his "first philosophy." And are we not justified in concluding that the crucial statements under examination contain irreconcilable if not contradictory views and that these are largely if not wholly traceable to the author's complete ignorance of the functions of the brain and nervous system?

While such a conclusion does not constitute final proof that had Aristotle known the function of the brain, of the cerebral cortex especially, he would not have believed in the separableness of the intellective part of the soul, it does show that he had advanced so far toward a wholeness, or unified theory of man that he could not depart from his general direction without contradicting himself. This reference to the idea that *entelecheia* would be applicable to man in the whole gamut of his individual life only by including its potentialities as well as its actualities (which, note, it does not include according to the statement under examination), is a point at which we may conveniently resume the examination of Aristotle's teaching concerning the appetative faculty of the soul as this manifests itself in the sexual mode of reproduction. This resumption is necessary to enable us to see still further what appears to be implied concerning a human being's wholeness in his teaching about sexual pleasure.

RELATION BETWEEN PLEASURE AND SENSATION—ANCIENT AND MODERN VIEWS

In resuming this examination we note first Aristotle's conception of pleasure. The phase of the conception that is specially relevant to the point in hand is stated, more or less clearly in various connections, perhaps nowhere more so than in the following: "Where there is sensation, there is also pleasure and pain, and, where these, necessarily also desire" (*De An.* 413^{b} 20. Smith trans.). The main point here is that sensation is always accompanied by pleasure or pain. And this clearly means (from what we have already learned) that the sensitive soul at least is always experiencing pleasure or pain. Satisfaction or dissatisfaction would probably come nearer to Aristotle's meaning. What immediately follows the passage just quoted, though bearing only indirectly on the main point, is important: "We have no evidence as yet about mind or the power to think; it seems to be a widely different kind of soul, differing as what is eternal from what is perishable; it alone is capable of existence in isolation from all other psychic powers." For us the most significant thing about this passage is the unmistakable evidence it furnishes of Aristotle's uncertainty as to whether the intellective part of the soul is or is not linked with the other parts—the nutritive, sensitive, and locomotive parts.

In the light of what the anatomy, histology, and physiology of the neuromuscular system and especially of the cerebral cortex, have taught us, Aristotle's uncertainty about the connection of the "mind or power to think," with sensation is only of antiquarian interest.

But what about the relation of sensation to pleasure and pain? Do we know for a certainty that "where there is sensation there is also pleasure and pain?" Still more, do we know that where there is pleasure or pain there is also sensation?

I fail to find anything in any of the works that seems to be an effort at a *direct* answer to these questions. But it matters little whether Aristotle made such an

effort or not, for we are sure he could not have made much real headway in it. The whole factual realm most directly involved is that of the minute structure and function of the sensory and motor nerve terminals, the details of stimulus and response, and so on.

But I believe that if we study his utterances about pleasure in the various works, particularly the long discussion of it in the *Nicomachean Ethics*, beginning the study with what he gives in the *History of Animals*, we may recognize that he laid the foundation for a conception of pleasure upon which a sound modern conception can be erected. The passage in the *History* specially referred to is:

> The life of animals, then, may be divided into two acts—procreation and feeding; for on these two acts all their interests and life concentrate. Their food depends chiefly on the substances of which they are severally constituted; for the source of their growth in all cases will be this substance. And whatsoever is in conformity with nature is pleasant, and all animals pursue pleasure in keeping with their nature. (589ª 5, Thompson trans.).

A student approaching Aristotle in the more usual way, namely, from the side of formal philosophy, is likely to question whether in such a statement the author really includes man with animals. Does he in very truth mean, for instance, that all man's "interests and life concentrate" on "procreation and feeding?"

We know well, from the *Ethics* particularly, that Aristotle was about as far as any philosopher or religionist, or any humanist of whatever school ever was, from viewing man as a "beast of the field" in the degradative sense often attached to that phrase. But from the context of this passage, it is impossible to avoid (if we wanted to) recognizing that man was here included in Aristotle's thinking as definitely and certainly as was any other animal. The passage is in the eighth book of the *History*, which may, I think, be fairly regarded as near the high-water mark of Aristotle's writing on natural history. For instance, his statement about nature's proceeding "little by little from things lifeless to animal life," that has become famous from being interpreted as adumbrative of the modern theory of Evolution, occurs in this book. But the opening sentence of the real discussion of the book tells us in so many words where the writer stands in the treatment.

> In the great majority of animals there are traces of psychical qualities or attitudes, which qualities are more markedly differentiated in the case of human beings. For just as we pointed out resemblances in the physical organs, so in a number of animals we observe gentleness or fierceness, mildness or cross temper, courage or timidity, fear or confidence, high spirit or low cunning, and, with regard to intelligence, something equivalent to sagacity. Some of these qualities in man, as compared with the corresponding qualities in animals, differ only quantitatively: that is to say, a man has more or less of this quality, and an animal has more or less of some other; other qualities in man are represented by analogous and not identical qualities: for instance, just as in man we find knowledge, wisdom, and sagacity, so in certain animals there exists some other natural potentiality akin to these. (*Historia*, 588ª 20–25.)

The unqualified animalness of man, though of a kind that is enormously high, relatively, in Aristotle's thinking, especially when he was at his best as a naturalist, does not, we hope, need further illustration.

Now can there be any other interpretation of the sentences quoted than that Aristotle conceived pleasure to go along with sensation to the very root-tips of animal life and so of the life of individual man? What else can be made of the statement that all animals seek pleasure "in keeping with their nature?" The real question here is as to the lower limit of sensation. So far as Aristotle's means of testing could go, he was probably justified in considering plants devoid of sensation.

In other words he was probably justified in considering sensation a strictly animal attribute.

Essentially the same idea is variously expressed by Aristotle. Thus: "Each kind of being, again, seems to have its proper pleasure, as it has its proper function,—viz., the pleasure which accompanies the exercise of its faculties or the realization of its nature." (*Nicom. Eth.* Book X, 5, 8. Peters trans.)

This identification of pleasure with the fundamental nature of living beings is so important for two hypotheses I am being led to, that we must consider its historic and factual backgrounds a little farther. Such consideration is needed in our day especially, perhaps, because of the wide currency of quite different interpretations and valuations of pleasure.

That Theophrastus, a pupil and disciple of Aristotle, should have had a similar view of pleasure is not surprising. "For Theophrastus," we read, "pleasure is the normal accompaniment of what is in accord with nature. As a rule, therefore, we take *pleasure* in things, since the common course of our functions is inevitably 'natural' and not antagonistic to nature" (Stratton, G. M., 1917, p. 48). And one passage from Theophrastus himself is much to the point because it is in refutation of a view of Anaxagoras which fits well with a brand of cynical philosophy quite prevalent in our era. "But as for the thesis," says Theophrastus, "that sense perception is universally conjoined with pain, this finds no warrant in experience, inasmuch as some objects are actually perceived with pleasure, and most of them at least without pain." (Stratton, p. 93). Those of our contemporaries who have been educated into a spiritual condition such that in whatever they do toward enjoying themselves they appear to get more misery than pleasure from it, will not of course be impressed by either the conception of pleasure that comes to us from Aristotle and Theophrastus, nor by a like conception from modern sources.

It is, however, important to note that many persons of this very day whose voices can hardly be ignored, have reached quite similar conceptions. A few of the many available references must suffice. "Simply to live, move and breathe, should be a delight." This statement by William James, is entitled to the more respect, I submit, from being quoted with approval by a foremost physician and contemporary worker in public health (Winslow, C. E. A., 1914, p. 214).

The only other statements I give space to are chosen for the trustworthiness of the authority, and, more especially, for the approach (as it seems to me) of the views expressed to the hypothesis to which we are coming.

We read: "Pleasantness might represent a general *organic state*, and unpleasantness the contrary state, each state being an internal bodily response to pleasant or unpleasant stimuli, and making itself felt as an unanalyzable compound of vague internal sensations." Again: "Pleasantness and unpleasantness are much less definitely localized [than sensations from the special senses]; they seem to be 'in us' without being in any special part of us." (Woodworth, 1911, pp. 175 and 174.)

What, we must now ask, do these diverse references to sensation and what is pleasant or unpleasant point toward? The literally immeasurable extent to which personal experience and common observation find sensation, feeling, emotion, passion and the rest, dependent on stimulations, external and internal, inevitably suggests that in seeking an answer to the question, the quarter to be turned toward first should be that of the most searching investigation yet made, on the dependence of the phenomena of living bodies on the responsiveness of the bodies to stimulations as just indicated. And thus we run head-on into one of the most recondite and also far-reaching divisions of psychobiology. It would be futile to attempt an exhaustive exploration of this realm here. But it would be equally futile to try to understand the issues most fundamentally involved without having got a firm hold on the main results of researches in the realm. So we must do our best under the circumstances toward such a hold.

Conformably with the historic diathesis of our enterprise we may first notice Aristotle's very wrong supposition that plants are devoid of sensation. The question of whether he did or did not produce a treatise on plants and the further question of whether, if he did, he could have still held to his error in this matter, it is useless to speculate on. For the error he certainly made and it appears to have influenced, willy-nilly, his whole theory of the activities of living beings.

Possibly, however, he was not quite so wrong as to facts nor unjustifiable as to theory as he seems. For in one passage at least he goes some distance toward recognizing the truth. After referring to the fact that various mechanisms may be injured or destroyed by being driven too hard in the performance of their normal function, he tells us: "This explains also why plants cannot perceive, in spite of their having a portion of soul in them and obviously being affected by tangible objects themselves; for undoubtedly their temperature can be lowered or raised." (*De An.* 424[a]

30 Smith trans.) It has been suggested that in the distinction here made between perceiving and being affected, Aristotle was aiming at an old and fanciful notion about feeling of joy and sadness experienced by plants.

The merest reference to what everybody knows about the Mimosas and other "sensitive plants;" about insect catching and devouring plants; and about the action of twining and tendril clinging plants, will suffice as familiar evidence of Aristotle's general wrongness in this matter.

Turning to the less familiar data, a summary statement may be made as follows: Some of the most exact and illuminating researches of the modern period into the "irritability," the "excitability," the "stimulability" (various terms meaning much the same thing) of living organisms, have been conducted on plants, nearly the whole range of the plant world having been requisitioned for investigative materials.

As a hint at what recent history presents in this realm, the work of the botanist Pfeffer may be mentioned. The choice falls here largely because this investigator's handbook is widely and favorably known.

In an interesting section of the book, entitled "nature of irritability," we read: "until recently the part played by stimuli in all vital phenomena has been overlooked." (Pfeffer, 1900, p. 13.) The date (1880) of the first German edition of this work indicates the extreme recency of scientific knowledge in this field. As the quotation clearly implies, when irritability at its base level is under consideration, there is no fundamental distinction between plants and animals. And since plants have no sense organs as this expression is usually understood, their sensitivity must depend on a very general property of living beings. So it happens that in the writings of the period of Pfeffer and a little earlier, one constantly finds statements to the effect that sensitivity, or responsiveness to stimuli, is a property common to all "living matter" or protoplasm. More recently results coming from increasingly refined investigation, the conception of sensitivity as a general property of "living matter" has had to be modified to the extent of recognizing that this general property belongs to living matter only as such matter occurs in organized, living bodies. Otherwise expressed, the concepts of both aliveness and sensitivity seem to be inseparable from the concepts of body and organization. The concept of "living matter," or "protoplasm" as a universal, undifferentiated, unorganized, uncorporealized something or other now stands in opposition to an enormous mass of observational knowledge, and is unsupported by a scintilla of such knowledge. The great science of living cells, cytology, is manifestly opposed to the concept. But cells are by no means the bottom link, so to say, of the chain of observational data opposed to the concept. In truth, cytological knowledge as now being developed, is concerned largely with a host of bodies of various sizes and shapes and degrees of permanence within cells. Although many, probably most, of these are not alive in the full sense, their undoubted contribution in one way or another to the vital activities of the cells is entirely at one with the conception of organized corporeality as a *sine qua non* to aliveness. "Living matter" has, in both fact and logic, about the same status that the term "humanity" has. There is no more justification in imagining "living matter" as someting apart from and antecedent to living bodies than there is in imagining "humanity" as something apart from and antecedent to, human beings.

"Protoplasmic systems" is a term now much used by investigators occupied chiefly with the functional, or physiological side of vital phenomena at these foundational levels. What the parts are and that the organization is, of these deep-level bodies, though of great interest, especially to experimental workers, can be merely touched here. Reference must be made to the great rôle the surfaces of the bodies play in the phenomena presented. "Surface layers" and "limiting membranes" are basic in the terminology of knowledge here. Now this character of these bodies lends itself well to the seeming necessity of extending our conception of sensitivity from our common experience as bodies to these deep-level bodies.

And we must notice that Aristotle, starting from this same common experience and applying to it principles of reasoning he himself had worked out, reached conclusions that are in striking general accord with the results of late experimental research. Thus on the problem of a living body and the matter of which it is composed, we read: "Now there is one class of existent things which we call substance [*ousian*], including under the term, firstly, matter [*hylen*], which in itself is not this or that; secondly, shape or form, in virtue of which the term this or that is at once applied; thirdly, the whole made up of matter and form." (*De An.* 412ª 2 Hicks trans.). Highly important is it that "shape or form" here is the crass thing of modern morphology. *Morphen kai eidos* is the original. That which is observed as well as the idea of it is what we have to do with. When we reflect that in the discussion of which this paragraph is a part Aristotle is dealing with the problem of the relation of the soul to the body; and note that the question of the "sensitive and appetitive faculties" was always conspicuously in his thought, we may well be impressed by the closeness of his approach, so far as theory is concerned, to the modern position.

There is now an enormous mass of evidence in court on the rôle of stimuli, excitors, activators in vital phenomena. This evidence makes it look as though we shall have to recognize that such phenomena are as dependent on externality for excitation to action as they are for building materials, and energy-yielding materials for action. But, of course, "externality" as thus used would include all such facts as that any part of our bodies, our kidneys, or gonads, or blood corpuscles, or cells generally, are external to all other parts. The conception of vital spontaneity, or automisity in a strict sense, in the sense, that is, of ability of a living body of any kind or grade either to begin its acts or to modify them, just by its own exclusive self, appears to be on the verge of elimination from sound biology. The following seems a fair epitome of conclusions under this head:

"As the facts stand at present there is no reason for assuming the occurrence of self-excitation. Spontaneity in a strict sense is far from being a demonstrated fact. One cell region, cell or cell group may of course excite another, but the original source of excitation and of the initiating energy is apparently always outside the region, cell or cell group excited." (Child, 1924, p. 186.)

What the relation of non-spontaneity as here indicated may be to the spontaneous, or autonomic atomic activity now receiving much attention by physicists, is an important but seemingly a very difficult question. The surmise that the doctrine of electronic indeterminism lends physical support to the doctrine of "free will" is easy to make, and seems congenial to certain types of feeling and thinking. And there may well be a connection of some recondite kind between the two. If men could discover scientific backing for an hypothesis that they are free to act in some such way as radio-active substances are, the discovery would be significant in several ways.

But it is important not to forget that the first mentioned doctrine is strictly atomistic-mathematical while the second is far from that. The "free will" is crasslymolar, i.e., corporeal, so far as all experience in exercising it goes. But still more important is it not to forget that both doctrines are items in, are products of, the knowledge-getting activities of human beings. The doctrine of atomic indeterminism is utterly dependent on *facts* that are molar or corporeal. These facts are, for one thing, the utter dependence of human observing, knowing and thinking on being humanly alive; and further the utter dependence of being alive on breathing, eating and the rest. Now these facts are so familiar as to tend to become just taken for granted. And things just taken for granted are, if highly complex, always liable to be treated as though some of their elements just do not exist.

Lillie has recently published some highly interesting discussions in this general field approached from the side of modern physiology (Lillie, 1931 and 1932).

"PLEASURE-PAIN" IN RELATION TO THE WHOLE-AND-ITS-PARTS

What, now, is the bearing of all this on the problem of pleasure? My answer is the first of the two hypotheses forecast several pages back. *Our experiences which we characterize as pleasant, agreeable, satisfying, and to which we give such names as pleasure, happiness, joy, are rooted finally in our sensitivity as bodies that are alive.* Their foundations are laid at the moment of our conception, are much advanced by being born, and are never entirely absent as long as we live. This deep-level truth gets partial recognition in the familiar statement, "Self-preservation is the first law of life." From the exercise by every living body of its ability to maintain itself we get such concepts and sayings as that it is "good to to be alive." Of prime importance is the recognition of that double dependence on externality (in part the "external world" of common experience) which our discussion focused attention upon. Each living body depends on what is external to it for its building and energy-yielding materials (air, food, water), and also for stimulation to all its activities. A body is alive in and of its ability to *use* for its own existence a relatively few things external to it, and to *respond to*, and *sense*, a literally uncountable number of things external to it. For a body to exercise these abilites is to fulfil, or to satisfy, its nature. These are the

very bottom acts by which a living body "makes good." This is as fundamentally true for the meanest weed-seed that in response to moisture and warmth pushes its sprout through the hard ground, as for the greatest human genius that responds to conditions of his time and surroundings and to his own internal "urge." Sensitivity and responsiveness as here touched upon, are basic to the concept of life-or-death needs and activities, and life-fulfilling needs and activities suggested elsewhere (Ritter and Bailey, 1927, pp. 268 and 295).

This tracing of the roots of goodness, the "good life," the "it is good to be alive," to their tips in the very deepest level of aliveness of animate bodies is likely to shock those who still cling to the teachings of traditional philosophy and ethics on this matter. For our interpretation gives no quarter to the idea of an ultimate, incarnate Good, that always, since Plato, has figured in one form or another in the theories of many delvers into the problems of human life. That is good according to this interpretation, which is intrinsic and essential to the very nature of the living being and is no outsider, or alien, even though its dependence on externality is irremovable. Goodness does not just come, somehow, from some imagined where, to occupy a portion (the spiritual or mental portion) of that relatively small group of living beings called human by us of the western world. "Good" though as deeply real as life itself, is not of extra- and supra-mundane origin in an imaginary Divinity. Rather, it is basic quality, or sense, inherent in, and essential to, the state of aliveness. Divine goodness is derived by abstraction from it; not it from Divinity.

(This is no treatise on the problem of Good and Evil. Since, however, we are led by the nature of this enterprise to touch the "good" aspect of the problem, I can not consistently leave the other, the "evil," aspect wholly untouched.

If my interpretation of "good" is right, it seems obvious that "evil" is not an antithesis of it in the sense that, for instance, white is the antithesis of black, or warm of cold. The existence of "good" is not conditioned on the existence of "evil" as its essential contrast. The antithesis of "good," is "dead," dead implying something that was alive but died. The "good" of a living being might, theoretically, experience little evil or even none at all.)

Now is there reasonable doubt that what we have here said is only a revision and extension in the light of present-day knowledge, of Aristotle's "where there is sensation, there is also pleasure;" of James' "simply to live, move and breathe should be a delight;" and Woodworth's "pleasure is 'in us' without being in any special part of us?"

From what we have already learned of Aristotle's inability, partly from lack of factual knowledge, to see how nutrition, sensation, and reason could be interdependent and inseparable parts of that particular complete reality which is a man, we readily foresee that he would be unable to go the whole way with moderns in interpreting the good. It is, however, to the point to notice that he recognized both the fallacy and the practical inadequacy of the "Universal Good" as conceived by Plato.

(It is worth while, historically, to recall that one of the most memorable of Aristotle's direct references to his disagreement with Plato is in this very connection. It is here that he tells us: "such an inquiry is not a pleasant task in view of our friendship for the authors of the doctrine of ideas." But further: "in the interests of truth, we ought to sacrifice even what is nearest to us, especially as we call ourselves Philosophers." (*Nich. Ethics*, I, [6] Peters trans.)).

Full of positive interest though Aristotle's discussion of the good, happiness, and the rest is as it comes to us in the *Ethics*, it would be out of place to pursue it here. One minor point, however, has considerable bearing on our interpretation. That is, the numerous passages that reveal his doubts about the relation between reason and the various feelings and emotions. One illustration must suffice. Although he sets it down positively enough, that as to man "mere nutrition and growth" must be excluded, elsewhere we read: "Good, then, is not a term that is applied to all these things alike in the same sense or with reference to one common idea or form. But how then do these things come to be called good? for they do not appear to have received the same name by chance merely." Then, of special interest comes the answer: "Perhaps it is because they all proceed from one source, or all conduce to one end." (*Ethics* I, 6). I am quite sure that "one source" and "one end" of this conditioned statement could without forced interpretation include the lowest level sensations in the one direction, and the highest welfare of the organism in the other, of our interpretation.

(I venture to remind the reader in connection with this suggestion of Aristotle's about source and end relative to what is good, or my account on earlier pages of my own experiences in orange-eating and institution-building.)

Like in purport to the above, but more definite is Aristotle's recognition that the good for man must be connected with his "purpose in life," as we say. "Man's good," says Aristotle "would seem to lie in his

function, if he has one. But can we suppose that while a carpenter and a cobbler has a function and a business of his own, man has no business and no function assigned him by nature? Nay, surely as his several members, eye and hand and foot, plainly have each his own function, so we must suppose that man also has some function over and above all these." (*Eth.* I, 7, 11.)

The "function over and above all these" is, as all familiar with Aristotle's supreme appraisal of contemplation would foresee, the "life of his [man's] rational nature." Whether he was right in this is, of course, not our concern here.

This rather long but far from exhaustive examination of Aristotle's admirable even though fragmentary knowledge of the truth about sensation, pleasure, the good, and happiness has seemed indispensable to the prosecution of our main purpose in this division of our inquiry concerning *entelecheia*.

We now return to the reason for this diversion. The reason was, it will be recalled, what we found Aristotle saying about sexual pleasure when he was at his best as a zoologist but ignoring almost entirely when he was an ethicist. And we need to recall still more definitely what made this matter seem important. It was his guess, perhaps taken from Hippocrates, that the *intensity of the pleasure of coition is due to the involvement of the whole organism in the actior*.

I wish to contend now that the revision of this guess in the light of modern knowledge as sketched in the foregoing discussion justifies the erection of the guess into a definite hypothesis, or perhaps more exactly, a definite theory; and further that if the theory is true, this aspect of sex constitutes one of the strongest manifestations of the wholeness in the sense of complete reality (*entelecheia*) of higher animal organisms. For notice the facts involved:

(1) The activity of copulation is linked with that of nutrition, in the two most basic of all vital phenomena for man and the vast majority of organic species. Those phenomena are of course, the maintenance of the individual and of the species.

(2) The excitation upon which the activity primarily depends is the most basic of all, namely that of physical contact, or touch.

(3) In accordance with the conceptions of excitation and sensitivity apparently necessitated by modern physiology, the sensation accompanying the activity reaches down to the same level of vital phenomena as do nutrition and metabolism. In other words, it reaches to every cell or part of a cell of the organism that is truly alive. That is to say, it reaches down to the simplest living units into which living bodies are resolvable.

Does it not follow, then, that if "good" as applied to living bodies means what numerous students have somewhat vaguely interpreted it to mean, and we have more definitely so interpreted it, that the Hippocratean-Aristotelian notion of the multiplication of the "good," the pleasurable, feeling, with the multiplication of the body parts involved, would be logically sound? But notice what one of the most fundamental improvements of this ancient theory affected by modern research, is essentially. Aristotle went far enough, we have seen, with such improvement to see that the intensity of the pleasure could not be due to the coming of the germinal material from all parts of the body. But remember his substitute—*pneuma*, spiritus!

It seems to me a fair question whether Aristotle does not come nearer (perhaps unwittingly) a unitary theory of the soul, and of soul and body, in connection with this very matter of sexual pleasure than in any other whatever. The question hinges on the rôle ascribed to the spiritus in coition and the views previously noticed that "intellect [nous] is not found to to cause motion apart from appetency." Still more definitely in this direction, recall the recognition that concupiscence, a kind of appetency, "may move a man in opposition to reason."

So we are led to conclude that the wholeness of an organism manifested in the ecstacy of sexual intercourse as this is experienced by man and the higher animals generally, *is one of the most characteristic and powerful items in the complete reality of these organisms*. The momentarily complete monopoly of consciousness at the climax of sexual ecstacy should probably be viewed as the very highest manifestation of the individual's conscious unifiedness and wholeness. But it is important to notice that the tendency of the emotions to such monopoly is not by any means restricted to the erotic emotion. Many forms of contest, for instance, both physical and mental, may go far in this direction. In all such cases there is ample evidence not only as common experience, but from scientific research, of the involvement of very many, if not all, parts of the organism.

EVERYTHING WHATEVER AND THE WHOLE-AND-ITS-PARTS

Finally we must face the supreme question of all: What did Aristotle do about his conception of *entelecheia* when he viewed it in connection with his conception of Everything that Is or Has Been or Will Be? For we may be sure, from what we have abundantly learned about the man that he was not of the kind either to fail to ask this question or to do something toward answering it. Naïve and philosophical naturalism (Ritter and Bailey, 1927, p. 7) were altogether too closely intermingled in him to permit him to overlook or shy at the question.

Almost the most famous (from the standpoint of traditional philosophy) of Aristotle's works, the *Metaphysics*, may be said to be devoted primarily to the problem of man's knowing the Universe, or Cosmos, approached from the direction of the Whole-and-its-parts.

But it is obvious that "our usual method of investigation" could not be followed here without far-reaching modification. For the "compound whole" to be analyzed into its "uncompounded elements" (Weldon, 1912, p. 2) was certainly not accessible by the principles of knowledge-getting he himself had so largely developed and utilized. The method could be applied with considerable success in getting knowledge of a State or a work of art, as a poem (this article, part I, QUART. REV. BOL., Vol. 7, p. 381), or a bedstead (p. 387), or a block of ice (p. 383), or a bit of air (p. 384). But when it comes to the universe—what is it compounded of?

Yet, there is the sun, the moon, the planets, the starry hosts and the arching sky, no less truly things of sense experience than men and fishes, the blue sea, and the solid earth. Surely the heavenly bodies are parts—*of something*—no doubt about that. But of what? What is the whole now? Can there be a whole so big and inclusive that there is nothing beyond it, nothing left out of it?

No one who has faced seriously the problem of the One and the Many, as even we moderns are perplexed by it, can be decently indifferent to the magnificent onslaught upon it made by the Greeks nor unsympathetically critical of their meager success.

The study of what Aristotle did on the problem may well begin with a reference to the view, now apparently accepted by nearly all students of the Aristotelian works, that the *Metaphysics*, the work in which the deepest meaning of existence is treated, was not called Metaphysics by its author. *The First Philosophy* was his name for it. The main significance of this is that really *Theology* would probably indicate more truly what Aristotle was here aiming at than does metaphysics as commonly understood. For he was obviously quite

as interested in the problem of God as in that of Nature.

The moment we touch the question of what the ancients (any of them) did on the problem of Everything, we are confronted with the pitiful meagerness (as compared with ours) of their factual knowledge. And much of this meagerness stands in relation to every division of nature, it stands with special importance in relation to the heavens. Undoubtedly facts in this subdivision are among the most striking and appealing of those in any subdivision whatever. But from their very nature they are specially beguiling to common experience. Wonderful, indeed, is the rising and setting of the sun, the effects of its radiations upon the earth, and the responses to these radiations by living bodies. Nevertheless so far as concerns all land-and-air inhabiting animals, man with the rest, problems of finding, securing and utilizing food and water are manifestly much more compelling of attention and goading to action. The growing light and warmth of each new day just comes, as free grace, to the mother bird and to the mother human. Nothing is exacted of the recipients—unless indeed the stimulus to song by the bird and prayer of thanksgiving by the woman be counted as exactions. How different the advent of the day for both creatures relative to food and drink! Be up and doing is the exaction now. For the final issue is Life-or-Death. And this must be met quite otherwise than by benisons of song or prayer, however desirable and good these may be.

So while perhaps knowledge of the heavenly bodies is the oldest branch of natural science, it certainly is not the oldest branch of practical experience. And who has a scintilla of real evidence of theoretic or speculative knowledge in any realm whatever that does not rest finally on practical experience, individual and racial? Herein matters of the most human concern are the issue.

History reveals that men in all ages have applied their imaginations and speculations to their free-grace experience of the heavenly bodies with the hope of gaining help therefrom in their active, often hard, experiences of such earthly bodies and situations as are Life-or-Death to them.

To this Aristotle was not apart from his predecessors and contemporaries. Indeed he seems to have been behind some of them in some respects. But we cannot in decency be unsympathetic with our forebears in this if due regard is had to the study of astrology among peoples and cultures down to our very selves with our daily newspapers as media for disseminating astrological beneficence and malevolence.

With reference to Aristotle's position in this, our concern is, as in other matters, quite as much with his *thinking* on the basis of the factual knowledge he possessed, as with his *knowledge itself*. And our main interest is, of course, the question of how his conception of *entelecheia* stood in relation to his conception of Everything, especially as to the heavens as a part of Everything.

It is significant that in the discussion which embodies his conclusions about the nature of the heavens and of God, Aristotle begins not with the heavenly bodies but with substances, matter and movements; and with actuality, potentiality, and cause. In other words he begins where common experiences begin, namely, with earthly things.

As an illustration of how objectively real he was, take this: "For how will there be movement, if there is no actual cause? Wood will surely not move itself—the carpenter's art must act on it." (*Metaphys.* 1071^{b} 28, Ross trans.). Then, true to his custom as a student of genesis, there immediately follows: "nor will the *menses* nor the earth set themselves in motion,

but the seeds must act on the earth and the *semen* on the *menses*." Philosophically sound but scientifically very unsound, you see—as in many other instances.

How then is man—individual man—whose existence is dependent on the act of procreation by a man and a woman, and on innumerable acts of his own in securing and utilizing nutritial substances, going to fully comprehend *Everything*? Sharply and certainly limited as a man is both in coming to be and in continuing to be, how can he encompass in any sense, by his own acts, even by his most powerful and comprehending acts, that which is entirely unlimited, so far as his experiences testify?

The evidence is conclusive, I believe, that on the whole Aristotle approached the great problem in this humanly animalistic fashion.

But what could he really do toward solving it? Almost absolutely uninformed as he was of the facts and interpretations on which the idea of organic evolution rests; pathetically meager as was his technical information about reproduction and development, about nutrition, respiration and metabolism, about neuromuscular action, and about the physics and chemistry of the external environments of living things, there was no course open to him by which he could advance beyond what others had done than to put enormous dependence, just as they had done, on thinking and reasoning about such observational data as they possessed. Now this sort of thing implied (as it always does) discussion, argument, speculation, disputation. It meant *dialectics*. And dialectics is a method of striving after truth that has had tremendous vogue in the history of human culture. And there is much uncertainty as to whether the balance sheet of such striving shows truth in the black or in the red.

A great part of Aristotle's effort to solve the problem of All Existence was made by applying this very method. There is no doubt about it. Accordingly his results have to be tested by this criterion as well as by that of observational procedure and inevitable logic.

Against the theory that number is in essence such as to be the sole explanation of Everything; also the theory that Platonic Ideas hold this exalted place Aristotle levels his great dialectic powers. How successful he was in this, I do not undertake to judge. Nor does it seem to me of much importance, its significance being historical and logical rather than factual. Of far greater importance is the circumstance that at a deeper, more vital level of his information and his thought, he recognized that a *sine qua non* to any body's having information or thoughts about the nature of numbers or of Ideas or anything else is the fact of being alive with all this implies as to begetting, birth, growth and differentiation; also as to many vastly complex organic functions especially of digestion and metabolism, and of sensitivity.

ARISTOTLE'S THEOLOGY AND ENTELECHEIA

That he tripped a few times when he tackled the problem of Everything, is not surprising. Indeed, all things considered, it is wonderful that he did as well as he did. And I venture to contend that his faithfulness, on the whole, to his conception of *entelecheia* was a major factor in his accomplishing whatever of value he did accomplish here.

Let us examine then the question of what exactly he did toward squaring this conception with his dialectical efforts. We can use Ross's *Selections*, (Ross, 1927) to good advantage. The group of selections combined by Ross under the title "Aristotle's Theology and Astronomy," begins (in keeping with what we pointed out

above about Aristotle's starting his discussion of the Heavens with his earthly experiences) as follows:

> Since there were three kinds of substance, two of them physical and one unmovable, regarding the latter we must assert that it is necessary that there should be an eternal unmovable substance. For substances are the first of existing things, and if they are all destructible, all things are destructible. But it is impossible that movement should either come into being or cease to be; for it must always have existed." (*Metaph.* 1071^{b} 1–7.)

This problem of substance is upon us here in its full terribleness. As for the two physical kinds, the material, or substratum of bodies, and the bodies themselves as forms, Aristotle was no more dubious about them than is any consistent modern naturalist (a naturalist, that is, who has not become sophisticated by atomistic physics). It was the third kind, the unmovable kind, that troubled him most. For even though he found great difficulty in making it fit with the other kinds, he still thought he could not get along without it. And one of the particularly hard questions in connection with it was that seemingly it must be non-sensible.

> "For (1) it [*Physics*] does not deal with the final cause (for this is the good, and this is found in the field of action and movement; and it is the first mover —for that is the nature of the end—but in the case of things unmovable there is no first mover), and (2) in general, it is hard to say whether the science we are looking for [i.e., "Wisdom as a science of first principles"] deals with perceptible substances or not with them, but with certain others." (*Metaph.* 1059^{a} 35.)

"Hard to say" indeed! For even today where are we with reference to it? Modern physics seems actually to have fallen behind Aristotle to the extent that it is now staking everything on atoms as experimental analysis knows them and as they can be treated quantitatively. Nor have the prevalent schools of philosophy, ethics and religion, made clear and solid advance beyond him. For in none of these has substance as form in the sense of Aristotelian morphology received any approach to adequate recognition. So the question we are asking boils down to this: What did Aristotle do toward reconciling his "usual method of investigation," i.e. of "analysing a compound whole into its least parts," with his supposed necessity in a particular case, that of the problem of Everything, of proceeding by the reverse method? For as we see, he really began with the parts (least parts?) in this case and tried by the dialectic method to conceive the whole. Here is a passage that seems to me to go far by implication toward answering the question.

Arguing for his theory of a "first" agent as the cause of all objectively known things he says:

> Since (1) this is a possible account of the matter, and (2) if it were not true, the world would have proceeded out of night and "all things together" and out of non-being, these difficulties may be taken as solved. There is, then, something which is always moved with an unceasing motion, which is motion in a circle; and this is plain not in theory only but in fact. Therefore the first heavens must be eternal. There is therefore also something which moves them. And since that which is moved and moves is intermediate, there is a mover which moves without being moved, being eternal, substance, and actuality. (*Metaph.* 1072^{a} 18–25 Ross trans.).

The crucial point here is that the last word of the passage is actuality (*energeia*) and not complete reality (*entelecheia*). May we not properly ask why this eternal substance that is insensible and is a mover of the heavens and all things without itself being moved, should not be characterized as complete reality and not merely as actuality? Is it not justifiable to conjecture that the remarkable something that Aristotle here assumed he at the same time felt to be lacking the sort of reality that led him to conceive *entelecheia*? Reflect on the incongruity of applying the same term to such a phenomenon as he supposed the

heavens to be, that he applied to the phenomenon of the transformation of ice into water (this article, part I, QUART. REV. BIOL., Vol. 7. p. 383) or of the change of a warm body to a cold one (p. 384).

The difficulty of observing all that is observable of any whole-and-its-parts, regardless of size and complexity, and of thinking about it, and being entirely consistent throughout, is undoubtedly very great. Yet that such consistency was what Aristotle really felt the need of when he conceived *entelecheia*, appears to me certain. How difficult it is to measure up to the aim is illustrated by the fact that Aristotle himself almost certainly fell short now and then. An important instance is furnished by his efforts in connection with this very problem of Everything. He, like innumerable other persons in many ages and cultures down to some of the distinguished astronomers and all of the astrologers of our own day, conceived the heavens to be the region which is the favorite, if not the exclusive abode, of the eternal and divine. With Aristotle movement was, as we have noticed, what he regarded as particularly convincing objective evidence of the eternal. For the dialectical proof of the endlessness of movement in a circle seemed to him conclusive. And since this was supposed to be the course traveled by the heavenly bodies, they furnished proof of eternality and divinity. His discussions of the heavens, especially in the *De Caelo* and the *Metaphysics*, make it clear that his interest here was at least as much theological as scientific. In these discussions he seems to flatly contradict his conception of *entelecheia* in one instance. And it is fair to guess, I think, that his contradiction is due to the dual interest indicated. The contradiction occurs in the following passage.

"Evidently there is but one heaven. For if there are many heavens as there are many men, the moving principles, of which each heaven will be one, will be one in form but in number many. But all things that are many in number have matter. (For one and the same formula applies to *many* things, e.g., the formula of man; but Socrates is *one*.) But the primary essence has not matter; for it is complete reality (*entelecheia*)." (*Metaph.* 1074ª 36, Ross trans.).

It is in connection with this passage that Ross writes me(without knowing of my reference to it here): "Yet for the most part Aristotle uses the words (*entelecheia* and *energeia*) interchangeably. Ross also writes "*Energeia* was not a well-established Greek work. It, no less than *entelecheia* seems to be a coinage of Aristotle. At least the new Liddell and Scott has found no earlier use of it." Such being the case, is not the question a fair one: Why should Aristotle have coined two such important words if they were quite interchangeable in his thought?

The statement that complete reality has not matter is unreconcilable, so far as I can see, with the many assertions, direct or by clear inference, that complete reality does include matter. The instance of ice and water, and of warm and cold referred to above may serve as examples in the purely physical (modern sense) realm. A footnote to this passage by Ross seems to furnish similar evidence for the biological realm. "The Logos," says the note, "is common to all men, so that it must be matter which gives Socrates his uniqueness."

It may be contended, and it is probably true, that the matter assumed to be responsible for Socrates' uniqueness is wholly different from the "primary essence," of the passage, and which is the "complete reality" of it. But this really helps to focalize my criticism. For evidence is unescapable that Aristotle was fully committed, as a biologist, to the idea that the germ of man as of organisms generally was potentially the organism in its actualized form so far. In other words, he was committed to the modern idea of heredity as to propagation, and to epigenesis as to ontogeny. More of the passage in *De Partibus* quoted (this article, part I, QUART. REV. BIOL., Vol. 7, p. 386) is especially clear on

the point: "Every germ implies two organisms, the parent and the progeny. For the germ or seed is both the seed of the organism from which it came, of the horse, for instance, from which it was derived, and the seed of the organism that will eventually arise from it." (*De part.* 641^{b} 33–37). This is, so far as matter is concerned, in accord with the fully established facts of genesis and is opposed to the theory of something, a primary essence, for example, in addition to the matter. For even the germplasm theory, though holding that the germinal substance does not really come from the parent, does not invoke an essence or first agent, or cause in the meaning of Aristotle when he departs from his usual course as a student of living nature and becomes genuinely metaphysical as a speculator about the heavens.

Attention may be incidentally called to the fact that the theory of germinal mutation of the ultra-orthodox school of genetics is perilously near, in logic, to the Aristotelian theory of a non-material first agent, or essence as the cause of the mutations, that is, of the source of new species.

Fortunately, as it seems to me, Aristotle connects his theories of thought and of motion in such a way as to give added justification to the view that he contradicts himself when he uses *entelecheia* in the instance cited. The passages I am about to quote will be more easily seen to bear on the point if read in connection with our examination of Aristotle's views about pleasure in relation to thought. Directly continuous with the passage quoted above and ending with "there is a mover which moves without being moved," etc., we have: "And the object of desire and the object of thought move in this way; they move without being moved. The primary objects of desire and of thought are the same. For the apparent good is the object of appetite, and the real good is the primary object of rational desire" (*Metaph.* 1072^{a} 26–28 Ross trans.). Then comes a long discussion of the intermingling of things distinctly earthy with things heavenly. In this discussion the concept of actuality (*energeia*) plays a large part while that of complete reality (*entelecheia*) plays no part at all. Some of the instances follow:

"Since there is something which moves while itself unmoved, existing actually, this can in no way be otherwise than it is. (1072^{b} 7) . . . On such a principle, then, depend the heavens and the world of nature. And its life is such as the best which we enjoy, and enjoy but for a short time. For it is ever in this state (which we cannot be), since its actuality is also pleasure (1072^{b} 15) . . . If, then, God is always in that good state in which we sometimes are, this compels our wonder . . . And life also belongs to God; for the actuality of thought is life, and God is that actuality; and God's essential actuality is life most good and eternal. We say therefore that God is a living being, eternal, most good, so that life and duration continuous and eternal belong to God; for this *is* God." (1072^{b} 25).

Another, more literal at least in part, translation of the last sentence is: "Now, our statement is, this,—that the Deity is an animal that is everlasting and most excellent in nature; so that with the Deity life and duration are uninterrupted and eternal: for this constitutes the very essence of God." (M'Mahon, 1910, p. 332). The passage in which this occurs is regarded by M'Mahon as probably "the most lucid statement of Aristotle's notions of the Divine Nature of the Being and Attributes of God."

That God is a *Zoon*, everlasting and very good; and that *Zoe* is life co-enduring with God is surely deserving of special attention as indicating the remarkable contrast between the Greek conception of animal and the well-nigh universal modern conception of it.

In the light of what has been already presented in this essay, is it any wonder that Aristotle should find no place for his conception of *entelecheia* in the reasoning these statements present? Surely, the conception of God as an animal in the sense that fish, bird, mammal, and man are species of the genus animal would be as con-

trary, even as repugnant, to the general spirit of the Aristotelian science and philosophy as to that of any system whatever of science, or philosophy, or ethics, or religion. Yet rigid adherence to the cardinal principles of biology and logic, of both of which Aristotle is the acknowledged founder, would seem to commit him to just that conception.

Such a passage makes one wonder whether it may have been written (or spoken?) long before its author had immersed himself in the study of living nature and of the psychical processes involved in the study, and that he never took it up again in earnest. The problem of the chronological order in which the Aristotelian works were produced, to which experts are now giving serious attention, shows its great importance in such passages.

If, then, my interpretation of Aristotle's system of knowledge and thought, and especially of his conception of *entelecheia*, is on the whole sound, the mental picture of the man and his system must be very different from that which seems to hang in the mental galleries of most of those in which there is any picture of him at all. For see what in barest outline such a picture portrays:

A man insatiably curious about, and determined to know and understand as far as possible, everything whether of earth or heaven.

Committed to the gigantic task thus implied and attending greatly to the psychical activities involved in the task, the man pictured blocks out for the earth as a compound whole the domain of natural science with a comprehensiveness and, for living things, a detailed truthfulness that no one before him had remotely approached.

For all the earthy portion of the task, the pictured man recognizes with remarkable insight that interpretation of a whole-and-its-parts requires a conception for which he coins the term *entelecheia*.

As to every aspect except one of the terrestrial portion of the task, he is presented as satisfied that his new term is applicable. The exception he seems to feel concerns the power of reason as possessed by a single animal species, man. Here, seemingly after considerable perplexity, he decides that his new term does not apply. This power is too great, too precious, too divine, to be included even in man's completeness as an earthly being.

Finally when he who is pictured comes to the problem of the heavens, the celestial position of his task, he appears still more perplexed as to the status of his new conception. Now we seem to see him falter between two stupendous alternatives: (1) Must he conceive the heavens as the abode of Deity—yes, in ultimate essence very Deity itself—to be entirely unknowable, inaccessible, to earthborn man? Or, (2) May he conceive the heavens and God not thus beyond the reach of man if man be conceived in one of his parts, his reason, to differ from himself as earthborn, in just the way requisite to give him the power of knowing God?

In neither alternative, notice, could *entelecheia*, earth-engendered as we have seemed to find it, be strictly and fully applicable.

MODERNIZED ARISTOTELIANISM

What, we may now properly ask, would be necessary to transform this mental picture of the relatively little informed ancient Aristotle into the picture of a modern man, enormously informed as he may be?

Can there be serious doubt as to what the revised picture would show? Recur to the problem of the soul as Aristotle struggled with it—the problem of whether there are three souls, or one soul with three parts, nutritive, sensitive, and rational. Examine this in the light of the principle of the whole-and-its-parts which Aristotle

himself clearly saw in many of its aspects. Examine it also in the enormous addition of light from present-day anatomy, physiology and chemistry of nutrition and of stimulus-response and the higher and highest mental activities.

From such an examination conjoined with a firm grip on what Aristotle aimed at by his term *entelecheia*, the conclusion seems to me unescapable that no such giant intellect as his is essential to see that the complete reality of a man *does* include his attribute of reason regardless of how varied, or subtle, or mighty it may be.

Now this is a way of saying that in inventing the word *entelecheia* Aristotle saw conceptually farther into the nature of man than the meager factual knowledge he possessed seemed to warrant.

Further, it is a way of saying that the dualism of Soul and Body, of Mind and Matter, as this has plagued much of human culture disappears when subjected to the joint light of the best Aristotelian thought and the best analytical results of modern research.

But if the revised picture would portray one eagerly willing to include his own highest, most cherished powers among his earthborn possessions, even more would it portray one willing to include the heavens regardless of how divinely conceived, as inseparably linked with the earth to constitute the Grand Whole of Things. For reflect on what the astronomy, the physics, and the chemistry of our era have done to confirm objectively not only Aristotle's reasoned conclusion that "there is but one heaven," but that this one is by no means utterly cut away and different from mother earth. Had Aristotle possessed as much observationally verified information about the movements of the earth, the other planets, and the stars; about gravitation; about the chemical composition of the sun and other heavenly bodies; about the nature of light and heat; about the electro-magnetic conditions of the earth and the sun; and about radiation, as enters into the common knowledge of our day, it does not seem to me at all probable that he would have staked as much as he did on the nature of the heavens in contrast with the nature of the earth, in constructing his theory of the eternal and divine—in a word, his Theology, And this is equivalent to saying, according to the viewpoint of this essay, that under such conditions he would not have hesitated to conceive *entelecheia* (Complete Reality) in such a way that it would extend to both Earth and Heaven and all that in them is, man and all his powers by no means excepted.

LIMITATION OF ENTELECHEIA

But even though *entelecheia* would extend thus far it would not imply coexistence with, for instance, the Hebrew Yahveh or the Mohammedan Allah. For these names seem to have been designed to be all other Names above; Names, that is, that should designate Being and Power adequate for all needs, desires, and hopes of human hearts and heads.

An extended discussion of the portion of Everything that extends beyond the meaning of *entelecheia* is for that very reason beyond the scope of this essay. One aspect of the problem may, however, be appropriately noticed to the extent of a few sentences.

It would seem that Everything Whatever would have to include all that now is, all that previously has been, and all that thereafter will or may be. In other words, All Existence thus conceived must be coextensive with Space and Time (as generally understood). Now due consideration for the principles of potentiality and actuality (and great consideration we have seen Aristotle giving them) seems to require the recognition that All Existence

Whatever would have to include not only all that has been or now is actualized of all potentiality, but all that might possibly be actualized. But the concept of *entelecheia* really includes, according to our interpretation, only so much of potentiality as has been and is now being actualized. That is to say any potentialities that a body or situation may have that never become actualities would not be included in the concept. To illustrate, any potentialities a human infant may possess which never come to actuality even though a well-rounded three score years and ten or more may be lived, would not be included in the *entelecheia* of that individual. By this view the "complete reality" of a human life, no matter how full of years and good deeds it may be, would not include those possibilities it had at the beginning that were never realized. Immeasurable (infinite?) potentialities or possibilities would therefore exist that would not come within the meaning of *entelecheia*, but would come within the seemingly intended meaning of the Divine.

So it results that enormously meaningful and useful as *entelecheia* is for natural science and natural philosophy, it falls short of adequacy in its application to All Existence Whatever. The "complete reality" of the concept is not complete enough, as one may say, to include fully that aspect of man's response to nature for which in all his ages and cultural stages he has sought satisfaction, and to the summed-up, abstracted object of which he has given the name God in one or more of its numberless synonyms and variants. The flood of stimuli from the external world that pour constantly upon us as raw material of emotional experience—how vastly this exceeds, we now realize, our ability to give rational, analytic attention to it in all its details!

Accordingly for an adequate understanding of man's response to Nature, especially in the emotional moiety of that response, we must go elsewhere than to the teachings of Aristotle. Or, perhaps more exactly, for such understanding the Aristotelian teachings must be amended and extended to make them conform with the information and ideas concerning Nature embodied in the world-culture of our era.

The presentation of what, so far as I have grasped it, the suggested amendment and extension would consist in, belongs to quite a different essay from this. Here a mere hint in that direction must suffice:

> In accordance with the psychologically well-recognized importance of distinguishing between any emotion as such, and the reasoned interpretation of that emotion, it turns out that the sharpest kind of distinction needs to be made between the mystic experience just as an emotion and the rational explanation of that experience. For such an explanation constitutes a theory, or doctrine commonly called mysticism. So mysticism has no necessary connection with religious experience.
>
> That emotional state, which is basic to the so-named mystic experience, can be entirely identified with out sensory responses to Nature and our absolute dependence on Nature. It can be shown that almost certainly the "sense of oneness with God," of the devoutly orthodox Christian; the "Substance," one and eternal, of Spinozian philosophy; the "immediate experience of the whole occurrence of Nature" of a professedly atheistic naturalist like John Burroughs; and "my mystic world lovely with trees and clouds and eddying streams I have never 'seen'" of sightless persons like Helen Keller, are responses to nature not so much in its infinity of space and time as in its organized completeness. By this interpretation Omnipotence is an attribute of Nature: Omniscience does not exist, because nescience is an attribute of a part only of nature, that is to say, of man. (Ritter, 1933 pp. 18, 19.)

Such an emotional-intellectual attitude toward Nature as that here sketched is one concomitant of the acceptance of the Universe, the phrase being understood as in this second Foreword.

Apparently this is the aspect of man's

response to nature that makes him "incurably," as the expression has it, religious just as, from another aspect of his response, he is "incurably" curious and intelligent; also scientific and philosophic.

That the world is vastly more indebted to Aristotle (and other Greeks) for logic, science, and philosophy, than it is for religion, should no longer stand in the way (as for centuries it has stood) of the world's entering into the enormous benefits of that indebtedness.

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OBSERVATIONS ON THE GENERAL BIOLOGY OF THE FLOUR BEETLE, *TRIBOLIUM CONFUSUM*

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INTRODUCTORY REMARKS

TRIBOLIUM CONFUSUM, the flour beetle, possesses characteristics which commend it to the attention of experimentalists. It is cosmopolitan and easily obtainable; it has a moderately short life-cycle; it is hardy and requires no elaborate equipment for its maintenance; both adults and immature forms are readily recoverable from the medium for study by a simple technique, and it spends its entire life-history in various pulverized grains such as flour. As has been pointed out before, this latter feature is particularly advantageous in many kinds of experimentation since, with a total environment such as flour, considerable stability of both intra- and extramedium relationships can be obtained. By using similar flour in all experiments and by expressing this flour quantitatively in terms of weight and surface exposure, as well as surrounding the flour by as constant external conditions as obtainable, a total environment can be established which is relatively quite constant and reproducible. Despite these attractive requisites it is only recently that *Tribolium* has come to the attention of biologists interested in theoretical problems. The economic zoölogists have been vigorously aware of the existence of this form for many years and have done much in providing data on the general ecology of the species. However, past this point, these workers, possibly unfortunately, have seemed more interested in devising methods of exterminating the form than recommending it to the attention of experimentalists. Babcock (1912), an early worker with *Tribolium*, used this beetle in studying the problem of metabolic water, finding the insect maintained a tissue water level about 40 per cent greater than the moisture content of an environment containing 10 per cent water. Davey (1917, '19), another early investigator, reported results showing that the length of life of these beetles could be prolonged if mass cultures received a small daily dosage of X-rays. Probably the principal credit in introducing *Tribolium* as an experimental organism, however, belongs to R. N. Chapman who in 1918 reported on its life-history, in 1924 on its nutritional requirements, and in 1928 on its suitability for population study. These papers will be discussed at length later: it is only important at this point to remember that they emphasized the practicability of *Tribolium* for experimental purposes by presenting a technique of handling the beetles as well as actual data. Since *Tribolium* does seem destined to merit more experimental attention, and since experimentalists need a factual background about the animal of their endeavors, it seems permissible at this stage to review fairly completely certain details about the flour beetle in the hope that such a review will be helpful and possibly stimulating to future investigators in the field. With this in mind the following general topics

about *Tribolium confusum* will be dealt with: first, taxonomic and historical considerations including a brief discussion of closely related species; second, a review of the food relationships of the flour beetle along with discussions of certain nutritional problems; third, a detailed discussion of the life-cycle and its relation to environmental factors; fourth, a discussion of productivity (fecundity and fertility); fifth, a treatment of some of the obvious behavior reactions of the beetle from both a statistical and an observational viewpoint, including some original observations upon cannibalism, copulation, and defecation; sixth, notes on the parasites of *Tribolium confusum*; seventh, a discussion of the technique of culturing and studying populations of the flour beetle, and last, a summary. It should be pointed out that no attempt will be made to cite or deal with every paper which has been written on *Tribolium*. This would be futile and not pertinent to the problem at hand due, in part, to the economic nature of many of the reports. The emphasis will be placed on a discussion of those investigations yielding information most likely to be required by future workers confined primarily to experimental research.

TAXONOMIC AND HISTORICAL

The genus *Tribolium* is a member of the Order Coleoptera, Family Tenebrionidae and sub-family Ulominae. The Tenebrionids are commonly referred to as the 'Darkling beetles' and include such forms as certain fungus-beetles, the pinacate-bugs, and the common meal worm *Tenebrio molitor* which has certain points of resemblance to *Tribolium*. There are several other species of *Tribolium* besides *confusum*. The other common American form is *Tribolium ferrugineum* Fab. Doctor Good, of the Department of Agriculture, informs me that the latter species is probably more properly referred to as *Tribolium castaneum* Hbst. These two American flour beetles seem to be remarkably similar in general structure and function. There are several criteria used by taxonomists in distinguishing the two from each other. *T. ferrugineum* = *castaneum* possesses a distinct three-jointed antennal club while *T. confusum* has a gradually enlarging club. Good (1933) points out the fact that the eyes of *T. confusum*, when viewed from the ventral surface, are smaller than those of the other species. The evidence at hand indicates that *T. confusum* has, in general, a more northern range of distribution than *T. ferrugineum* = *castaneum*. This paper will deal entirely with *T. confusum* largely because more is known of its general ecology. It should be emphasized, however, that the other species would probably lend itself as well to experimentation.

At the present time the genus *Tribolium* is very widely distributed over the world. It is largely disseminated in grains transported by commerce. The exact origin of the grain dwelling habit is not known but a probable interpretation is advanced by Good (1933: p. 328) who says,

> Almost without exception, the beetles of the subfamily Ulominae, of which Tribolium is a member, occur either as pests of stored products or else under the bark of trees and in rotting logs. It seems evident that all members of this group originally lived in the latter habitat and have recently adopted the flour-feeding habit. Two species of Tribolium, *T. madens* Charp. and *T. indicum* Blair, are found almost exclusively in such situations, and the two flour pests *T. confusum* and *T. ferrugineum* are themselves occasionally found there.

Tribolium confusum was really first described unknowingly by Etienne Mulsant who, in attempting to improve upon the account of the already known *Tribolium ferrugineum*, published a description of *confusum*. However, since Mulsant thought he was simply redescribing *T. ferrugineum* his reference to *confusum* has been ne-

glected. The credit of the description has been ascribed to P. N. Camille Jacquelin du Val who in 1868 recognized *Tribolium confusum* as a distinct species and published his account. Due to the fact that du Val's (now commonly written Duval) description typifies much of the taxonomic work of his era it is considered permissible to append it below as it existed originally.

(1) *Tribolium confusum*, Jacq. du V.

T. ferrugineo simillimum, et descripto ulterior supervacua. Ab illo differt capite latiusculo, genis ad oculos destinctius angulatis, pronoto postice leviter sed destincte sensim angustato, cum angulis posticis acutiusculis, interstitiis elytrorum paulo magis elevatis et praesertim antennis sensim clavatis, —Long. 0,0034-42.—

Je dois ajouter pour compléter la distinction des deux espèces en question que, chez le *T. ferrugineum* la tête n'est pas plus large que longue et offre des joues très obtusement angulées auprès des yeux, le pronotum est légèrement et régulièrement arqu suré les côtés, n'est pas plus large ou même est un peu plus étroit en avant qu'en arrière et offre des angles postérieurs droits, mais sans former de pointe saillante, enfin les antennes sont terminées par une massue tres distincte et assez brusque de trois articles, tandis que chez le *T. confusum* la-massue est insensiblement formée par les 4 ou 5 derniers articles. L'espèce que je viens dec signaler se trouve confondue dans diverses collections françaises avec le *T. ferrugineum*, et a été decrite pour cette dernière par M. M. Kuster et Mulsant. La phrase suivante, "*Antennae articulis tribus perfoliatis*," insérée par Fabricus dans son *Supplementum Entomol. Syst.* p. 179, nous fixe sur l'espèce de cet auteur. Le *Colydium castaneum* de Herbst appartient aussi à n'en pas douter à cette dernière. Quant à l'*Ips cinnamomea* de Herbst, la figure qu'en donne cet auteur démontre que ce ne peut être un *Tribolium* ni même à mon avis un Ténébrionide." (p. 181)

FOOD AND NUTRITION

Tribolium confusum lives in almost any kind of flour, cracked grain, breakfast food or meal. A list of specific foods in which these beetles are found has been compiled by Chittenden (1896, 1897) and includes whole-wheat flour, bleached and unbleached white flour, bran, rice flour, rye flour, corn meal, barley flour and oat meal. Good (1933) also reports the beetles living in chocolate, spices (red pepper), various kinds of nuts, and sometimes feeding on specimens in insect collections. Chittenden has found *Tribolium* in snuff, orris root, baking powder, ginger, slippery elm, peas and beans. The beetles are unable to feed on whole grains, as pointed out by Chapman (1931), because their mouthparts are not adapted for attacking large, hard pieces of food. Typically, the entire life-history of a *Tribolium* is passed within its original environment.

Chapman (1918) has studied the relative susceptibility of wheat flour to the invasion of *Tribolium* in order to determine, as he states it, if there is any 'preference' exhibited by adult beetles for specific flours. This investigator divided an experimental jar into five equal portions each containing a different kind of wheat flour which varied from finely ground flour (first middlings) to bran. These flours were in continuity with each other so that a beetle could pass from one type to another. Then adult *Tribolium* were introduced into the center of the jar, and, being surrounded by equal parts of the five types of cereals, were allowed to migrate. After a varying length of time the flours were examined and a census made of their beetle population. Chapman found that the bran contained essentially twice as many beetles as did the other media and concluded that these data indicated a preference selection on the part of *Tribolium*. In repeating the experiment with larvae no evidence of such a 'preference' was found. Observations were also made with rice flour, rye flour, barley flour and corn flour and the results of these experiments, in harmony with the first ones, indicated that the beetles were reacting, not necessarily to the richest food, but to the most coarsely ground medium. Chapman ingeniously tested this further by running a varying series of sawdusts

differentially ground and again corroborated the fact that the beetles react positively to the coarse material which they can enter readily and move through with facility. However, after the experiments had run a longer time it was observed that all of the flours, whether coarse or fine, were equally populated. This was due to the fact that the beetles eventually honeycombed the finely ground flour with tunnels and were then able to move freely through it.

Chapman (1924) reared *Tribolium* on various media and concluded, first, that microörganisms did not play an important part in their nutrition; second, that the nutritional requirements for growth, or increase in size, were less exacting than those for maturity or metamorphosis, and third, that wheat germ most closely satisfied these physiological requirements. Sweetman and Palmer (1928) investigated the vitamine requirements of *Tribolium*, and found that Vitamine B was necessary for their development. Their investigations showed that the wheat embryo was a rich source of Vitamine B. The conclusion to be drawn on the basis of these experiments seems to be that flours represent a well balanced diet and that *Tribolium* is typically able to live in them with its complete nutritional requirements well satisfied.

Under the caption of nutritional effects it might be interesting to mention briefly certain experiments of Holdaway (1933) dealing with the effect of starvation on sex ratio. This investigator subjected first instar larvae to different degrees of starvation as follows: the first group served as controls and were not starved; the second group received one day of starvation; the third group two days starvation, and the fourth group three days starvation. These experimental groups were incubated until the pupal stage was reached at which time sex was determined. The larvae starved one day exhibited a higher proportion of males than did the controls. This proportion did not exceed the limits of random sampling and can not be taken too seriously. However, the groups starved two and three days showed a statistically greater number of females than the controls. This was a significant difference. Holdaway shows that this alteration of sex ratio can not be explained on the basis of a differential mortality existing between the sexes and concludes that the starvation is the critical factor.

Certain other observations have been made on the physiological effects of various kinds of foods upon *Tribolium* and these will be discussed in later sections of this paper.

METAMORPHOSIS AND LIFE-CYCLE

Many references to the life-cycle of *Tribolium confusum* may be found in the literature. Most of these reports are incomplete, abbreviated, and frequently incorrect. In most ways, the best statements on the subject are those of Chapman (1918), Brindley (1930), Holdaway (1932), and Good (1933). These papers all present the results of original observation and do not resort to the recopying of often erroneous textbook statements. In the present discussion on *Tribolium* metamorphosis and life-history the information will be drawn largely from these four papers. For a succinct account in German the papers of Zacher (1927, '33) and Kunike (1931) can be recommended. Other pertinent references are those of Chittenden (1896) and Dean (1913).

As is typical with holometabolous insects, *i.e.*, forms displaying complete metamorphosis, *Tribolium confusum* possesses in its life-cycle, egg, larval, pupal, and adult stages. In order to clarify this discussion it is deemed wise to describe briefly these various types.

The eggs are ovoidal in shape when naked but usually appear somewhat irregular due to the fact that they are surfaced with flour particles which adhere to the sticky egg membrane. Brindley (1930) reports the following egg measurements: mean width, .40 mm., mean length, .64 mm., with a standard deviation of .02 and .04 mm., respectively. Chapman (1918) draws attention to the fact that the contour and general appearance of the eggs varies somewhat according to the type of medium in which they are found. Thus eggs in finely milled wheat flour would appear smaller than those from coarser cereals. Stanley (1932) determined the mean moisture content of 30,000 eggs and found it to be 44.9 per cent.

The eggs hatch into small, white larvae which measure, according to Brindley, on the mean 1.18 mm. in length, and 0.18 mm. across the head capsule. The first instar larvae weigh 0.028 mg. The moulting behavior of the larvae may be described in the words of Chapman (1918, p. 75) who says,

> For a short time before each moulting, the larva is inactive and the body is large in proportion to the head. The skin splits dorsally over the head and thorax, and the larva emerges. It is at first white, like the larva of the first instar, but after twenty-four hours it takes on a yellowish color. Immediately after moulting, when the larva has expanded as a result of being freed from the old skin, it has often been observed to remain quiet for a time.

An interesting question with regard to *Tribolium* metamorphosis is that dealing with the number of larval instars. Chapman and Brindley, both reporting original observations, have noted that there are six larval instars in the development. These writers measured the larval head width during the different instars and used this as a criterion of development: i.e., a certain head width indicated a specific larval stage. Good (1933) studied the number of instars by counting the number of exuviae (larval skins) deposited in the flour after each larval moult. This author concludes (p. 331): ". . . . the writer has determined that there is no fixed number of larval moults, but that the number ranges from 6 to 11 or more and is normally 7 or 8 instead of 6. This variation is due both to external conditions, such as food, temperature, and humidity, and to individual characteristics entirely apart from external influences."

The larvae gradually increase in size with every moult: Brindley shows the fourth instar larvae, for example, to have

TABLE 1

Larval measurements of Tribolium confusum. (Taken from Brindley by permission of the Ann. Ent. Soc. Am.)

INSTAR	AGE IN DAYS	LENGTH		HEAD CAPSULE WIDTH		WEIGHT
		Average	Standard deviation	Average	Standard deviation	
		mm.	mm.	mm.	mm.	mg.
1	0	1.18	0.05	0.18	0.01	0.028
2	3	1.64	0.11	0.22	0.03	0.035
3	6	2.38	0.08	0.29	0.01	0.119
4	9	3.23	0.20	0.40	0.01	0.332
5	12	4.00	0.44	0.53	0.04	1.09
6	15	6.00	0.70	0.69	0.03	2.40

a mean length of 3.23 mm., a head breadth of .40 mm., and a weight of .332 mg. The last instar larvae are 6.0 mm. long, .69 mm. broad across the head, and weigh 2.4 mg. As the time for pupation advances these last instar larvae become more and more quiescent and contracted and finally pupate. An original drawing of a last instar larval form is appended as Figure 1. Table 1 gives Brindley's measurements of *Tribolium* larvae.

The pupae of *Tribolium* are naked and, with the occasional exception of a slight abdominal movement, they are inactive. They are whitish-yellow when first formed

but turn yellowish with age, being brown at the time of emergence. Brindley finds the pupae have a mean length of 3.46 mm., and a width of 1.12 mm. There is a tendency for the female pupae to be longer than the male. A question of great practical importance for experimentalists working with *Tribolium* is the method of determining the sex of the beetles. As far as is known, the only reliable external sexual characteristic for any stage is found in the pupal stage. When the ventral posterior ends of the male and female pupae are examined under low magnification this sexual distinction is obvious. On the terminal segment the female has a pair of small appendages whch are reduced to indistinct elevations in the male. Figure 2 brings out this distinction between the two sexes.

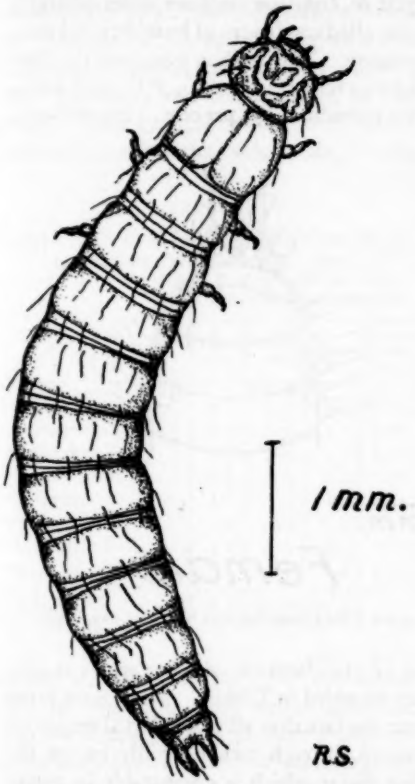

FIG. 1. DRAWING OF A LAST INSTAR LARVAL FORM

Marshall (1927) has described the development of the compound eye of *Tribolium confusum*, and, since this is a contribution to the metamorphosis of these beetles, it is considered wise to outline the essentials of this development here. The compound eye makes its first appearance in a late larval instar as a grouping together, in the eye regions, of a few hypodermal cells to form a spindle-like body. These cells are the visual cells and by increasing in size they form the retinula. At the distal end of this retinula four nuclei enlarge and, with their surrounding cytoplasm, become the crystalline cone cells. With development there is an increase in the size of these four cells and they eventually become the largest part of the ommatidium. In their growth they push the distal portion of the retinula away from the eye surface. At this stage there have been formed two distinct parts to each developing ommatidium: a proximal portion consisting of the retinula, and a distal portion composed of the crystalline cone cells. Pigment is laid down first in the visual cells, then in the corneal cells, and finally in the cells between the ommatidia. Each crystalline cone cell possesses a large clear space in its proximal part which is an anlage of the crystalline cone. In *Tribolium*, however, this cone disappears during the pupal period and is absent from the eyes of adult beetles. Early in its development each ommatidium becomes convex over its exterior surface and pushes out the cuticula covering it. This is the start of the cuticular lens. The cells of the cornea lying between the ommatidia secrete cuticula which moves inwards to the crystalline cone cells and forces them from their original position. With this secretion of cuticula the lens development is completed.

A good idea of the proportions of *Tribolium confusum* adults may be obtained from Figure 4. Brindley records their mean length as 3.4 mm., width across the thorax 1.02 mm., mean male weight 1.48 mg., and mean female weight 1.78 mg. There seems to be a quite consistent tendency for the females to be larger than the males but there are enough exceptions to this fact to prevent the size of the adults from being used as a reliable criterion of sex. Immediately after emergence the chitinous exoskeleton of the adult beetle is soft; the four months. The present author has observed that the majority of individuals which successfully emerge and meet with no accidents live at least six months. This long life is, of course, frequently valuable from the viewpoint of the experimentalist.

Brindley (1930) has reported on the life-cycle of *Tribolium confusum* under carefully controlled conditions of humidity and temperature. This author subjected the beetles to a temperature of 29.7° C., and a relative humidity of 73 per cent. His findings,

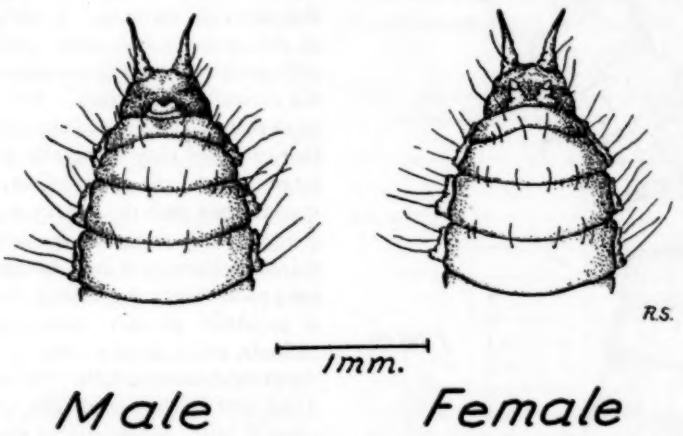

FIG. 2. TERMINAL VIEW OF MALE AND FEMALE TRIBOLIUM PUPAE SHOWING THE SEXUAL CHARACTERS

forms are inactive, and are a light brown in color. In one or two days, however, the beetles have assumed the typical reddish-brown color with the exoskeleton quite hard. In very old *Tribolium* the adults are nearly black. Good (1933) reports observations on the longevity of adult *Tribolium*. This investigator observed 50 individuals and found 13 of them alive after 24 months of life. Dr. Good has informed the writer that of these 13, one male lived approximately three years nine months, another male three years eight months, and a third male three years

as to the duration of the various stages, are recorded in Table 2. These data bring out the fact that all of the larval stages are passed through rather rapidly except the last instar which is comparable in extent to the egg and pupal periods. As indicated by the standard deviation this last larval instar also displays the greatest time variation in its duration. It is interesting to note that this is related to the observation of Chapman (1918) who points out that the last larval instar is more influenced by ecological changes, with respect to its duration, than any of the other

developmental stages. The average length of time required for the completion of a *Tribolium* life-cycle under Brindley's conditions of temperature and humidity, and food is 30 days. These data of Brindley's demonstrate that for a particular set of conditions, and a presumably in-bred stock, the variation in the length of the cycle is surprisingly small for observations based on 100 different individuals. It would be erroneous to generalize, however, that 30 days represents the usual length of the cycle. Good (1933), to illustrate, shows that the length of the cycle varies, for one thing, according to the food the larvae feed on. At 27°C., this writer finds that the larval period lasts 31 days when the forms had been reared on middlings as compared to 89 days when the larvae had been raised in white flour. The latter observation differs from the experience of the present author, however, since, in white patent flour at a temperature of 28°C., and a relative humidity of approximately 50 per cent, an entire life-cycle from egg to adult is completed in about 50 days. The cycle becomes longer as the temperature lowers. Good reports that *Tribolium* adults spend the winter in unheated Maryland flour mills in a semi-dormant condition, resuming breeding as the spring approaches. The larvae and pupae do not seem so able to withstand low temperatures as do the adults. Chapman (1931) reports that *Tribolium* adults die in a few weeks if subjected to a temperature as low as 7°C., indicating their inability to assume a true dormancy. On the basis of experimentation (Chapman, 1931; Chapman and Baird, 1933; Stanley, 1932) the relation of temperature to the length of the life-cycle can be summarized as follows: at 32°C., the development from egg to adult is passed through in about 27 days; at 27°C., in about 37 days, and at 22°C., in about 93 days. These figures were all obtained under constant conditions of humidity (75 per cent) with whole wheat flour used as food.

TABLE 2

Length of the stadia of Tribolium. (Taken from Brindley by permission of the Ann. Ent. Soc. Amer.)

STAGE	LENGTH OF STAGES (IN DAYS)			
	Minimum	Maximum	Average	Standard deviation
Egg	5.5	5.5	5.5	0.00
1st. instar	2.0	2.0	2.0	0.00
2nd. instar	2.0	3.0	3.0	0.05
3rd. instar	2.0	3.0	2.5	0.68
4th. instar	2.0	3.0	2.7	0.55
5th. instar	2.0	3.0	2.8	0.36
6th. instar	5.0	7.0	5.5	0.71
Pupa	6.0	7.0	6.2	0.44
Totals	26.5	33.5	30.2	

An essential point to be emphasized about the life-cycle of *Tribolium confusum* is that it must be expressed relative to the specific environmental conditions obtaining at the time of observation. Although no actual data are available as yet it is most probable that the duration of metamorphosis also varies according to the genetic pattern of the beetles as well as with the surrounding ecological conditions.

Holdaway (1932) has reported at length upon the effects of atmospheric moisture on *Tribolium*. This author deals with the following aspects of the subject: first, the effect of humidity on the size of adult populations; second, the rate of population growth as related to humidity; third, the effect of change of humidity on population equilibrium; fourth, the relation of humidity to the rate of development and the viability of the various metamorphic stages, i.e., physiological effects of humidity, and last, the importance of humidity in affecting entire populations. Holdaway finds adult *Tribolium* increase in numbers as the humidity increases from 25 per cent to

75 per cent. In his experiments there were 9.5 beetles per gram of flour at 25 per cent R. H., 11.4 beetles per gram of flour at 50 per cent R. H., and 15 per gram of flour at 75 per cent R. H. Above 75 per cent the number of adults declines due to the development of fungus in the flour. The author also noted that the adult populations kept at 75 per cent R. H. had a faster growth rate than those at lower humidities. By taking, for example, an asymptotic population of adult beetles characteristic of a 25 per cent humidity environment (9.5 beetles per gram of flour) and subjecting this same group to a humidity of 75 per cent the population assumed the concentration typical for a 75 per cent humidity (15 beetles per gram of flour). This indicates that the effects of humidity on the life-cycle are probably not irreversible in nature. In studying the relation of humidity to viability of immature stages Holdaway found that, in general, more eggs hatched in low humidities than in high. On the other hand, the larvae showed a greater survival in the high humidity environments with the optimum at 75 per cent. The pupae had coefficients of survival similar to those of the eggs since more pupae failed to develop as the surrounding atmospheric moisture increased. A consideration of the effect of humidity on rate of metamorphosis showed that the eggs and pupal stages were little influenced by humidity conditions while the larvae developed more rapidly as the humidity increased from 25 per cent to 75 per cent. Holdaway investigated the influence of 25 per cent, 50 per cent, and 75 per cent relative humidity on oviposition and concluded that no apparent effects were discernible. A further statistical analysis of his data (Table XV, p. 296), however, suggests that the fecundity rate is significantly higher for beetles in a 50 per cent humidity environment when compared with those in 25 per cent. The females in 75 per cent humidity also produced significantly more eggs than those in 25 per cent. When the females of the 50 per cent environment were compared with those in the 75 per cent environment there was a suggestion (1.9 times the standard error) that the former were ovipositing more rapidly. The effect of humidity on oviposition can not be considered definitely closed at the present time.

As an elaboration of the life-cycle of *Tribolium confusum* it might be well to describe briefly some of the facts known as to the growth of entire populations of these forms. Chapman (1928) has experimentally approached this problem with interesting results. This author set up six environments of whole-wheat flour which increased in size geometrically. The environments consisted of 4, 8, 16, 32, 64, and 128 grams of flour. Adult *Tribolium* were introduced into these environments to provide initially one beetle to each two grams of flour. Thus the four gram culture contained one pair of *Tribolium*, the eight gram culture two pairs, the 16 gram culture four pairs, the 32 gram culture eight pairs, the 64 gram culture 16 pairs, and the 128 gram culture 32 pairs. Counts of the number of eggs, larvae, pupae, and adults were made at various intervals in order to see how the total population was increasing relative to environmental size. The essential point of the data, as emphasized by Chapman, was that after a period of approximately 100 days the populations all reached an equilibrium which was similar for all the environments when measured in terms of beetles per gram of flour. In other words, the culture of 128 grams of flour with its initial population of 32 pairs of beetles, although much larger after 100 days in terms of total number of individuals, contained essentially the same number per gram of flour as did, for

example, the 4 gram-1 pair unit. Chapman calculated this equilibrium point attained by *Tribolium*, under these specific conditions of temperature, humidity and kind of flour, to be 43.97 ± 2.88 individuals per gram of flour. Chapman considers this equilibrium to be the result of the interplay between the capacity of the species to reproduce (Biotic Potential) and the resistance of the environment. The author concludes that cannibalism is a most important factor in this resistance since, although thousands of eggs may be present in the environment at any one time, only some of them escape being eaten. It is this eating of eggs, which is directly proportional to the population concentration, that maintains the equilibrium of the total number of individuals. Nicholson (1933: p. 140) criticizes Chapman's method of calculating 'environmental resistance' as follows:

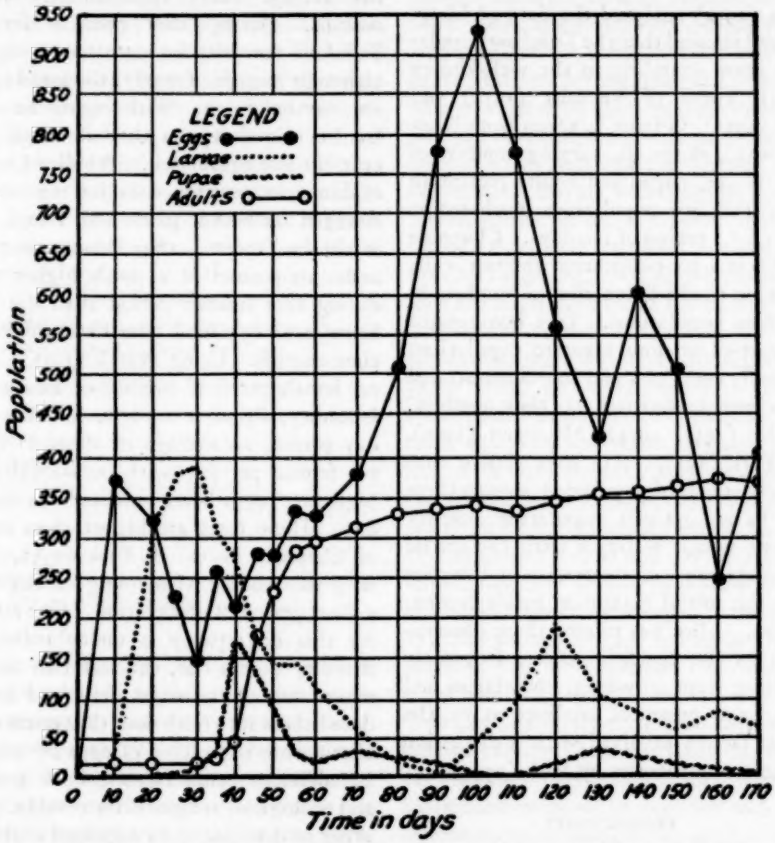

FIG. 3. POPULATION GROWTH OF TRIBOLIUM CONFUSUM AT 27°C., AND 75% RELATIVE HUMIDITY
(Taken from Stanley, 1932, Canadian Journal of Research, 6, p. 668, by permission)

. . . . Having experimentally determined the density of *Tribolium confusum* under certain conditions, he pro-

ceeds tc calculate environmental resistance by means of his formula. A glance at this example (p. 110) shows that the only use made of the experimentally determined density is to multiply *both* sides of the equation by it. Clearly the result is independent of density, and Chapman's hypothesis completely fails to deal quantitatively with animal populations.

Gause (1931) analyzed the data of Chapman and showed that the *Tribolium* populations grew according to the well-known logistic curve of Verhulst (1838) and Pearl (1925). Figure 3, taken from Stanley (1932), shows the varying numbers of eggs, larvae, pupae and adults present in active *Tribolium* populations incubated at 27°C., for a period of 170 days. Chapman (1933), in a pre-publication abstract, calls attention to the fluctuation of numbers in *Tribolium* populations. This investigator has carried on four separate populations for nearly two years and has demonstrated a synchronous fluctuation of their numbers.

Allee (1931, 1934), Maclagan (1932), and Park (1932, 1933) have shown that in early stages of population growth there may be an optimal population size for growth which is larger than the initial minimum beetle concentration and smaller than the initial maximum beetle concentration. Allee has related these observations to the beneficial effects frequently resulting from crowding. Maclagan and Park have presented analyses as to the casual factors involved with a discussion of these two theories by Park (1934).

PRODUCTIVITY

One aspect of the general biology of *Tribolium confusum* of obvious importance to experimentalists is the question of productivity. This can be interpreted as dealing with both oviposition, or fecundity, and fertility, or percentage of egg hatch. It is only recently that data have been reported on these considerations and much still remains to be done. Good (1933) reports a series of interesting observations on oviposition which are valuable since they record the number of eggs produced by females during the entire period of their egg-laying life. This period, according to Good, may last as long as 14 months, the average being approximately nine months. During this time a female *Tribolium* normally lays 400 to 500 eggs, although records of nearly a thousand eggs are not unknown. With regard to egg fertility Good reports that about 90 per cent of these eggs hatch. Brindley (1930) obtained oviposition data for ten newly emerged *Tribolium* pairs and found, as might be expected, that these young females oviposited at a much higher rate during this limited period than did the forms used by Good who obtained long-time records. Using Pearl's (1922) 'rate per female per day' method of measuring fecundity, Brindley reports, for this ten day period, an average of about 11 eggs per female per day with occasionally as high as 18 eggs being produced in a single day. These rates are higher than those of Chapman (1918) or Park (1932, '33) who also worked with young females over a short period of observation. One reason for this discrepancy is undoubtedly, as Brindley points out, the fact that he removed his adult beetles daily and introduced them into fresh flour thus essentially eliminating the eating of eggs by adults. In addition, other factors, both genetic and ecological, are quite conceivably operating in this case. As a general example, temperature has been demonstrated to affect oviposition. Stanley (1932) summarizes certain data of Chapman on the effect of temperature on egg-laying. At 22°C., the mean oviposition rate per female per day was 1.9 eggs; at 27°C., 6.24 eggs, and at 32°C., 10.73 eggs. These are all significant differences and clearly indicate the relation of moderate temperature varia-

tions to fecundity. Park (1932, '33), in studying population problems, had occasion to repeat frequently a standard experiment in which either one pair of young adult *Tribolium* or else a single fecundated female spent eleven days in 32 grams of patent flour at 28°C., with egg counts made at the end of that time. As a further contribution to the question of *Tribolium* productivity all cases falling within the above experimental type have been collected from the data of both papers and are biometrically presented in Table 3. These rates are, on the mean, slightly higher than those presented by Good but much lower than those of Brindley and Chapman as reported by Stanley (1932). It should be emphasized that in these experiments no attempt was made to eliminate cannibalism: the rates presented here are not therefore *sensu stricto* absolute fecundity rates but simply measure the eggs remaining in the population after some have been consumed. The emphasis here was placed upon the study of natural populations in which egg-eating is an important function. Park used patent flour in his experiments while Chapman and Brindley used whole-wheat flour. Good (1933) has shown that the kind of flour influences egg laying. At 27°C., in whole-wheat flour he found the mean egg rate per female per day was 2.43 eggs; in bran, 1.26 eggs; in oatmeal, 1.04 eggs, and in white patent flour, 0.58 eggs. There is, in all probability, no real discrepancy between these data and those of other authors since the experimental conditions and procedure have been different with various ultimate aims in mind. On the other hand, there seems to be no patent biological reason why these rates and those obtained by Chapman and Brindley, who worked with different stocks and in a different location, should necessarily closely approach the same constant figure. This argument is further substantiated by the investigations of Good (1933) who obtained oviposition rates much lower than those of Chapman. It should be indicated that Good purposely ran his experiments over a long period of time and the rates he obtained may have been influenced by aging on the part of the beetles. On the other hand Good removed the adults from their flour daily so that egg-eating effects were minimized. About oviposition Good says, "The number of eggs laid per day is not large. In no case were more than 13 viable eggs laid in one day by a single female, and the average was only 2 or 3 per day. Under optimum conditions Brindley records 18 eggs in one day and a much higher daily average than is indicated here." (p. 330)

TABLE 3

Egg productivity rates of Tribolium confusum (237 cases calculated from the data of Park, 1932, '33)

Maximum number of eggs after 11 days incubation	63. eggs
Minimum number of eggs after 11 days incubation	0.
Mean number of eggs after 11 days incubation	38.9 ± .64
Median number	31.2 ± .80
Standard deviation	14.8 ± .45
Coefficient of variation	38% ±1.17
Mean rate per female per day	3.5

As can be seen from the standard deviation of Table 3, these particular egg-rates of *Tribolium* vary considerably between individuals even when experimental conditions are quite closely controlled. These data do not, of course, indicate whether the egg-laying and egg survival variation is due to non-controlled ecological factors, or to genetic differences between the beetles. Presumably both types of factors are operating. It is a common observation that one female may have a high oviposition rate which persists throughout a considerable portion of that individual's life, while

another female, under as nearly similar conditions as can be obtained, has a consistently low oviposition rate. Facts such as these seem to indicate that the genetic constitution of the beetle in question is an important factor in fecundity as well as are ecological influences. With respect to egg fertility Park (1933) found that about 90 per cent of the eggs produced larvae providing they were laid by a female who had experienced more than a single initial copulation. Park also reported the oviposition rate of young virgin *Tribolium* females showing it to be much lower (about 20 times) than that of fecundated individuals.

An interesting, but anticipated, feature of *Tribolium* oviposition is that it is affected by certain environmental influences such as kind of flour, possibly humidity (Holdaway, 1932), temperature, conditioning of the flour by beetles living in it (Park, 1934), as well as population density relationships. Facts such as these where such a major function as reproduction can be shown to be sensitive to experimental changes enhance the value of the form for study. More work on this general subject seems forthcoming judging from advance abstracts presented by Chapman (1933) and Chapman and Baird (1933).

CERTAIN ASPECTS OF TRIBOLIUM BEHAVIOR

Certain observations on the behavior of *Tribolium* may be of interest. From common laboratory experience it is obvious that the adults are photo-negative: i.e., they loosely cluster in shaded portions of a container when subjected to direct light. In connection with their behavior to light it is instructive to know that *Tribolium* cultures can be maintained indefinitely in complete darkness as Chapman (1931) has noted. These beetles also react negatively to gravity and when located on a nearly vertical surface crawl to the top of the container. This gravity reaction seems to be markedly displayed, however, only when the beetles are not located in some kind of solid culture medium. In the latter medium the tendency to vertical stratificaton, at least in small environments, is slight.

In moving through the flour the beetles seem to employ only trial and error methods since their movement appears quite at random. This fact is possibly implied in the common name of the form, the 'confused' flour beetle, and has been observed a number of times. Stanley (1932, 1932a), working with *Tribolium* populations mathematically, makes the random movement one of his fundamental premises. Park (1933) has shown how the adults of a single colony of *Tribolium* were distributed entirely at random. This type of movement seems to be the logically expected one since such an environment as flour would probably limit the efficient operation of such senses as visual and olfactory. This is substantiated by Chapman's (1931) observation that *Tribolium* exhibits relatively little olfactory selection of food. Although all the evidence points to complete random movement by *Tribolium* in flour, at this stage, however, it can not be too dogmatically asserted that the beetles never exhibit any directed motion.

The fact that adult and larval *Tribolium* practice cannibalism has been reported by several authors. It is known that eggs, early instar larvae, and pupae are consumed. Chapman (1928) emphasized the importance of the egg-eating behavior in the maintenance of population equilibrium. Park (1933) experimentally obtained egg-eating rates by introducing a single adult beetle into an environment of 32 grams of flour which contained initially 30 eggs and making counts of these original 30 eggs after 11 days of incubation. It was found that virgin females, fecundated females, and males had a statistically similar egg-eating

rate as tested by this experiment with the mean number of eggs eaten for the period approximately seven per beetle. It is to be expected, of course, that with other types of experiments these rates might change relatively.

Outside of these statistical approaches to the study of cannibalism, however, there seems to have been little done on the observational side. It might be interesting at this point to resort to natural history and describe the behavior of an adult *Tribolium* in the process of eating an egg. It is impossible, naturally, to observe the beetle eating an egg when both are covered with flour. Therefore, an observation container was fitted up which consisted of a petri dish lined with filter paper containing a number of eggs and several adult *Tribolium*. To facilitate the observation of details the whole dish was placed on the stage of a low powered binocular microscope and the beetles examined through this magnification. In this dish the beetles move at random without any visible reference to the eggs. They occasionally walk over them or roll them to one side as they move about but seem to engage in no definite preliminaries previous to the actual consumption of the egg. It is not clear exactly which senses are employed in the location of a specific egg which is to be eaten. Presumably both visual and tactile senses aid the beetle in its reaction. In eating the egg the *Tribolium* follows a rather definite behavior pattern. The egg is grasped between opposing surfaces of the tarsal joints of the prothoracic legs with the tarsal claws usually puncturing the egg membrane. While the egg is being thus grasped the prothoracic legs are extended forward so that the femur and most of the tibia of both legs parallel the longitudinal axis of the body and may be partly covered by the head. As a result of this leg movement the part of the egg proximal to the beetle is brought into contact with the mouth parts. By means of mirrors it was possible to observe the mouthparts in action. The short, horny mandibles seem to function primarily in fragmenting particles from the egg while the maxillae and maxillary palpi aid in this fragmentation and also work the material into the mouth proper aided by the labium and labial palpi. The eating of the egg is facilitated by the fact that upon puncturing the egg membrane the fluid contents coagulate and, mixing with the flour which adheres to the external surface of all *Tribolium* eggs, produces a mixture of sufficient consistency to be handled by a form possessing biting mouthparts. As the beetle consumes the egg the prothoracic legs pull the remaining portion posteriorly so that it always approximately maintains its same position relative to the mouth. The mesa- and metathoracic legs usually brace the beetle by extension to the side and back. A general representation of the beetle eating an egg is depicted in Figure 4. It is not the purpose of this description to suggest that the behavior described is a fixed and non-variable one, for this would be fallacious. This report does describe, however, the typical chain of events as they were observed during the reaction. A beetle can completely consume an egg in fifteen minutes, although frequently it may take much longer. Quite often a *Tribolium* may leave an egg before it has been completely eaten. When the observation tray was covered with a thin layer of flour the egg-eating behavior remained unchanged.

Another behavior reaction of probable interest to experimentalists is the copulation one. The importance of copulation in certain *Tribolium* populations has already been reported by Park (1933) where it was demonstrated that, up to a certain point, the fecundity and fertility of *Tri-*

bolium females was increased by repeated copulations. Copulation seems to be a frequently practiced behavior among *Tribolium* as both statistical and observational data show. There do not seem to exist, however, any particularly elaborate or both as to its extent and nature. Often a male may follow a female and, attempting to mount her with no success, eventually cease his activities. Again the two sexes may meet and copulate without any visible preliminaries at all. In mounting the fe-

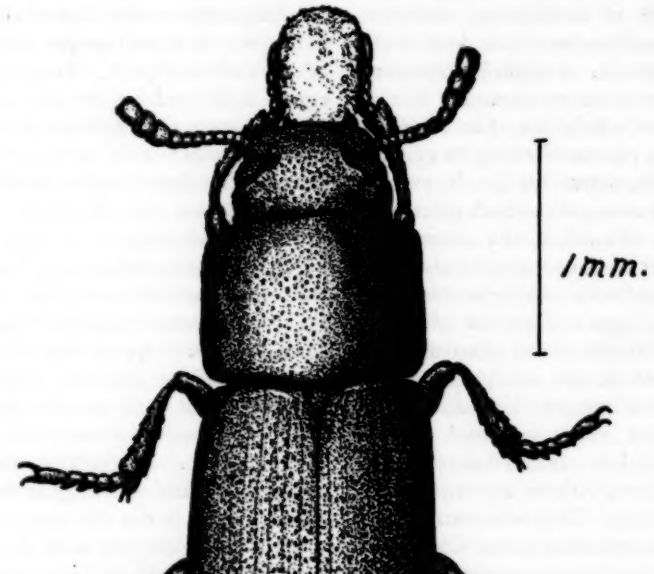

FIG. 4. DRAWING SHOWING AN ADULT TRIBOLIUM EATING ONE OF ITS OWN EGGS (from life)

unvarying behavior reactions associated with the process. Males can occasionally be observed to attempt to mount other males. This suggests that sex recognition is not highly developed among these forms. The pre-copulation behavior varies greatly male the male beetle clasps her with all three pairs of legs extending them around the ventral surface of her body. The male lies somewhat posteriorly on the dorsal surface of the female so that his head coincides with the thorax of the female. This

allows the terminal end of the male's abdomen to project out behind that of the female. The chitinized penis is then projected downward and forward making contact with the vagina which is but slightly extruded. When this connection has been effected the seminal fluid, a mucous-like, slightly milky appearing secretion, enters the vagina. As far as the author's personal experience is concerned the mating pair may remain united for as long a period as fifteen minutes or, conversely, the copulation may be consummated within a space of two or three minutes time. During the copulation the females are sometimes observed to move about rapidly and at other times to remain motionless. Frequently the male completely withdraws the penis but remains clasped to the female and inserts it again after a varying length of time.

A brief comment might be made about the defecation of *Tribolium* adults. This was observed by means of the same type of observation apparatus used in studying cannibalism. Defecation seems to be a rather frequent process judging from the accumulation of fecal matter on the filter paper. The feces are well formed, ovoidal in shape, and vary in color from almost colorless, through various degrees of green, to a dark black. The beetles observed had all been fed upon patent flour. In defecating a single pellet is usually extruded. In the cases observed this is rather quickly done. The beetles may defecate either while moving or quiescent.

Another behavior reaction may be mentioned since, although infrequent, it is of considerable biological interest. Chapman (1926) noted that when aggregated *Tribolium* adults were stimulated by rubbing they emitted a rather pungent gas, "....which smells not unlike an aldehyde. It irritates the mucous membrane of the nose and turns flour and certain other materials pink, and in high concentration, affects the eyes." (p. 295) When the larval and pupal forms of *Tribolium* were subjected to the gas Chapman observed that in about 10 per cent of the cases abnormal forms were developed. The mature larvae emerged with a combination of larval and pupal structures and the pupae emerged as adults displaying some deformity. Chapman concludes that the development of these monsters is associated in some manner with the gas produced by the beetles when the latter are irritated by some unusual stimulus. The control larvae and pupae not subjected to the gas showed no incidence of this atypical metamorphosis. Doctor John Stanley (personal communication) has advised me that there is some evidence that the gas, along with certain fluids found in *Tribolium*, may be irritating to man resulting particularly in gastric disorders. He advocates discretion in the handling of the form for this reason.

Adult *Tribolium confusum* possess fairly well developed wings which leads to the question concerning their ability to fly. Good (1933) reports that he has never observed *Tribolium confusum* fly or even attempt flight. According to this author, however, the closely related species, *Tribolium ferrugineum=castaneum*, frequently attempts to fly and, when stimulated by a strong light, occasionally makes short flights of a few feet. Holdaway (1932) reports that on one occasion only has he observed *Tribolium confusum* fly and that was when the form had been stimulated by high temperature. The present author has never observed flight on the part of these beetles.

Adult *Tribolium* frequently practice death-feigning or letisimulation. In handling the beetles the author has often observed them draw in the legs and remain completely quiet despite the fact they were being moved about. This reaction is of short duration, however, a few seconds

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at the most, and the forms soon become active irrespective of outside stimulation.

TRIBOLIUM PARASITES

Good (1933) reports the occurrence of two mites *Acarophenax tribolii* Newstead and Duval and *Pediculoides ventricosus* Newport, and a bethylid (parasitic wasp), *Rhabdepyris zeae* Waterston as parasitic on *Tribolium*. This author states that these three forms are not very effective in controlling the beetles. Doctor Clay G. Huff, of the University of Chicago, and the present author studied a case of internal parasitism in *Tribolium confusum* which arose in the stock cultures. The parasites seemed to have their effect upon the pupal stages since the latter, when parasitized heavily, grew dark in color and failed to emerge as adults. Histological preparations were made of the pupae and the parasite studied. The conclusion was reached that this form was a coelomic coccidian probably belonging to the genus *Adelina* and previously referred to by Riley and Krogh (1922) and White (1923). The latter writer mentions that infected larvae and adults, as well as pupae, frequently die of the disease. Gregarines are also reported as infesting *Tribolium confusum*.

TECHNIQUE

In culturing stock *Tribolium* populations practically any type of receptacle may be used. Standard glass battery jars about 25 x 19 cm., are convenient and economical and may be stacked in incubators with a minimal loss of space. Many kinds of food can be used for breeding the beetles and most of the *Tribolium* workers seem to have favored whole wheat flour. Since the wheat husks so frequently resemble the larvae and since they (the husks) do not pass through fine meshed sieves it is advisable to use a flour which has these seed-coats removed. The present author uses 'Ceresota' a non-bleached, commercial, white patent flour. It is preferable to keep the stock jars in constant temperature incubators at a temperature between 27°C. and 30°C. The humidity can either be accurately controlled or may be allowed to vary between 40 and 75 per cent. In order to maintain the stocks at asymptotic concentration it is necessary to change the flour frequently as Park (1934) has demonstrated.

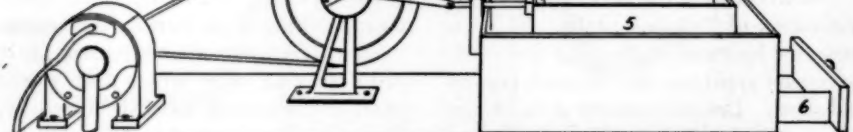

FIG. 5. DIAGRAM OF THE AUTOMATIC FLOUR SIFTER
1, belt wheel; 2, driving wheel; 3, driving rod; 4, flour sieve; 5, flour sieve holder; 6, removable collecting tray

A census can be taken of infected flour by passing it through fine meshed standard silk bolting cloth. Chapman (1918) used different meshed cloths and found that number 9, for example, would not pass any of the *Tribolium* stages but would permit finely milled flour to go through; number 3 separated eggs and larger larval stages from the flour; number 000 passed all eggs and larvae except the last instar forms, etc. Thus by using bolting cloth sieves of a specific mesh *Tribolium* stages may be re-

tained for counting as desired. To facilitate the sieving of the flour the author uses an electrically driven sifting apparatus constructed for him by Mr. M. E. Carson, Whitman Laboratory, University of Chicago. This apparatus (Fig. 5) simply moves a framed piece of bolting cloth back and forth over a removable tray and separates the desired beetle stages from the flour which collects in the tray below. There is evidence to show that shaking affects adversely the oviposition of adult *Tribolium* (Stanley, 1932). For this reason, it is desirable to gently remove the adult beetles from the experimental flour before sifting is started. The beetles may be handled with brush or spatula.

SUMMARY

The twofold purpose of this paper has been to point out the desirable features exhibited by *Tribolium confusum* Duval for experimentation, and to present information about the general biology of the form which might be useful to present and future investigators. At the present time most of the *Tribolium* experimentalists are using the flour beetle in population studies. This is, of course, easily understood since the form is admirably suited for studies of this type due to the homogeneity and reproducibility of its environment. Many population problems remain to be solved here, however, and future work is to be encouraged. On the other hand, *Tribolium* does not seem limited to population studies alone; investigations on nutritional, radiation, behavior, metamorphosis, and possibly genetic problems can be approached by the use of this organism.

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A PHYLOGENETIC ASPECT OF THE MOTOR CORTEX OF MAMMALS

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FOREWORD

THE following paper was left in manuscript form by my colleague, the late Dr. Ernst Huber. A greatly condensed and abbreviated digest has already appeared (in chapter III of "Evolution of Facial Musculature and Cutaneous Field of Trigeminus. Part I," QUARTERLY REVIEW OF BIOLOGY, vol. 5, pp. 133–188, 1930). Dr. Huber had intended that, before publication of the present contribution, he would include full discussions of the corticospinal tract in the various mammalian groups, but this plan was never completed. As it stands, the paper is practically as written by Dr. Huber, although I have made certain changes that seemed desirable. I have also attempted to include some of the more recent literature dealing with the motor cortex, and am entirely responsible for the discussions of those investigations published since 1929. The part dealing with man was lost by Dr. Huber and has been rewritten by me in accordance with notes left by him. I have taken the liberty of inserting synonyms for the various primates discussed at the proper places in the text; these may be of value to the reader in fully identifying the forms studied. Figures 1, 2, 7, 8, 10, 11, 12, 17 and 19 were drawn by Dr. Huber and published in his aformentioned paper, and they are reprinted here in slightly altered form. The remaining figures have been selected from the literature by me and have been most kindly redrawn by Mr. James F. Didusch. I am entirely responsible for the figure explanations, as well as for the brief "Introduction." It should be mentioned that the discussion of the motor cortex, as defined in this paper, does not include those stimulable areas of the frontal and occipital regions which give rise to ocular movements; these centers differ histologically from the immediate precentral motor area and hence fall into a separate category. No systematic discussion of the cyto-architecture of the cortex has been attempted, but references are made to this phase of the problem wherever they seem pertinent. WILLIAM L. STRAUS, JR., Feb. 21, 1933.

INTRODUCTION

A specialization within the telencephalon to form a "motor cortex" is an attribute of the mammalian brain. The formation of such an area is foreshadowed in reptiles (alligator: Johnston, 1916; Bagley and Richter, '24; Bagley and Langworthy, '26; turtles (*Chelydra, Chrysemys, Cistudo*): Johnston, '16; lizard (*Gerrhonotus*): Johnston, '16) and birds (pigeon: Rogers, 1922/23), at least some of which possess electrically excitable cortical areas. In these forms, however, the axons of the stimulable cortical cells do not form a corticospinal tract. This tract is found only in the mammals (Kappers, 1920). The "motor cortex," as defined in this paper, is that cortical area or areas giving rise to such a direct pathway to the various body levels. Hence a discussion of this system is necessarily limited to mammalian forms. The present paper attempts to bring together the existing knowledge concerning the motor cortex in the various orders of mammals, with a view toward demonstrating its phylogenetic development.

1. DIFFICULTIES OF CORTICAL STIMULATION

Since the epoch making pioneer work of Fritsch and Hitzig (1870) a vast amount of data from studies on the motor cortex of the brain in a number of mammalian types (monotremes, marsupials and placentals) has been accumulated. The extensive literature on this subject is, however, of very unequal value. Unfortunately, numerous errors have often been carried over into later publications.

In spite of the fact that Fritsch and Hitzig used galvanic current with bipolar stimulation, their first delineation of the motor

cortex was remarkably accurate. Ferrier (1873) introduced the faradic current as being preferable to the galvanic stimulation of the cerebral cortex. Though his results were far less accurate than those obtained by Fritsch and Hitzig (1870) and Hitzig (1873, '74, 1904), the new method eventually proved superior to the old. It was generally accepted and made possible a decided advance.

Much of the success of cortical stimulation depends upon proper anesthesia and application of moderately weak current. No definite result can be expected when animals are too deeply anesthetized, because under such conditions the strong current necessary to elicit movement may spread into areas adjacent to that under direct stimulation. Many of the incorrect and unreliable statements in the literature are obviously the result of such faulty technique. Ferrier, working on adult animals, and using a very strong current, scattered the motor areas over almost the entire lateral surface of the cortex. Partly because of the same reason, a series of succeeding authors obtained scarcely more accurate results.

It is essential to realize that the strength of current necessary to elicit movements in the newborn and in animals in the very first stages of postnatal life is greater than that required to produce movements in adults of the same species. As the animals grow to maturity the threshold for the excitability of the motor cortex gradually becomes lower. Leyton and Sherrington (1917) reported comparative data from cat, macaque and chimpanzee, animals which are known to give ready and consistent response. These three types were stimulated side by side under the same conditions, and were found to be similarly responsive. Yet from a comparison of the results which the various investigators obtained in stimulation experiments on many representatives of the various orders and suborders of mammals, it is seen that apart from those differences in the excitability of the motor cortex which are due to variable factors in connection with the experiments (e.g. condition of the animal, condition of the brain, depth of narcosis, etc.), there appear to exist additional consistent differences presumably dependent upon dissimilarities in structure and function of the motor cortex of various mammals. Thus mammals with primitively organized brains require stronger current than those with more highly organized motor cortexes. Likewise, the strength of current necessary to elicit movements varies in regard to the different centers of the motor cortex of the same animal. Because of these various conditions a keen judgment, gained only by much experience, is necessary for the application of the proper optimal current.

Furthermore, if one critically analyses the records of bilateral and ipsilateral responses, he must conclude that possibly many of them were the result of spread of current along the dura mater to cranial and upper spinal nerves of the side under stimulation. In some instances, however, especially in mammals with a primitive central nervous system, such bilateral and ipsilateral responses may have an anatomical basis in the incomplete crossing of the corticospinal tracts. In other instances, however, as in part of the facial musculature, bilateral response with prevalence of the contralateral side appears to be due to bilateral cortical representation of certain muscle groups which normally give purposeful automatic movements of the ipsilateral side associated with movements of the contralateral side (synkinesis). Such bilateral cortical representation is evidently confirmed in some instances through ablation experiments, e.g. in the case of the m. orbicularis oculi of the chimpanzee (Grünbaum and Sherrington, 1904).

On the other hand, animals have often

been stimulated under too light anesthesia or without any anethesia whatever. Under such conditions, movements occurring spontaneously in the animals and not the result of electrical stimulation have frequently been recorded together with the movements resulting from actual stimulation. Some examples of such errors which are scattered throughout the literature are pointed out in the following pages. For an experienced and critical investigator it should be possible to discriminate between the two kinds of movements, especially in adult animals, because each has its characteristic attributes. If an investigator has acquired this discriminating judgment he may in certain instances obtain more complete results without the use of anesthesia (compare e.g. results of stimulation experiments on newborn and very young animals).

The application of pharmacological agencies for increasing the local excitability of the motor cortex may likewise at times have led to false impressions.

The introduction of the unipolar method by Sherrington (1893) brought about a considerable improvement in cortical stimulation. This method makes possible a more accurate localization. It has proved to be of greatest importance in investigations of highly organized brains with a highly differentiated and much subdivided motor cortex. Occasionally, nevertheless, more recent investigators have preferred the bipolar to the unipolar method, with fair success in lowly mammals (e.g. Rogers, 1924, on the opossum).

As a whole, the results obtained by the different authors are proportionate to application of proper methods, careful operative technique, good condition of the experimental animal, proper care of the exposed cortex, accuracy of observation, discriminative judgment and a series of other factors.

The results so far at hand are as yet little correlated with the genealogy and behavior of the animals investigated. A vast amount of data concerns merely the determination of the extent and location of the responsive motor areas and of their subdivisions. Yet even in regard to the topography of these areas relatively few mammalian types are satisfactorily known.

II. THE ONTOGENETIC DEVELOPMENT OF THE MOTOR CORTEX

The facial area has been only roughly determined in connection with the general investigations on the motor cortex. From these investigations it becomes evident that the motor facial area was one of the first during the phylogeny of mammals to become definitely localized in the cerebral cortex.

During the ontogenetic development of certain mammals, however, the facial area becomes responsive to electrical stimulation at a considerably later stage than does the fore limb area. This is clearly shown through the important contributions of Weed and Langworthy (1925) and Langworthy (1927a). Using the opossum (*Didelphys virginiana*) as an experimental animal, these investigators stimulated various stages of pouch-young, which correspond to early fetal stages of placental mammals. Already at the early age of 23 days (crown-rump length, 33 mm.), the youngest stage used in the experimentation, fore limb movements were obtained upon cortical stimulation, while no other areas responded.

Through the observations of Hartman (1920), it has become known that after the 10 day gestation-period the very immaturely born young, measuring only 11 mm. in crown-rump length, crawl, without any assistance from the mother, from the vaginal orifice to the pouch, where they quickly become attached to the nipples. In crawling they use the fore limbs, swaying the head as far as possible to the side opposite the hand which is taking

the propelling stroke. While the fore limbs already are used at this early embryonic stage in such an important function, the hind limbs are represented merely by extensive buds. The kangaroo young reaches the pouch of the mother in the same manner as does the opossum (Hartman). In their structural and functional development the fore limbs precede the hind limbs, not only in the polyprotodont marsupials, which in the fully developed stage have fore and hind legs of corresponding size, but likewise in the more specialized diprotodont marsupials, not excepting the big kangaroos with their exceedingly large, strong hind legs and contrastingly small, short fore legs. It was not until the stage of 76 day old pouch-young opossums of 127 mm. crown-rump length that Weed and Langworthy obtained movements from centers other than that of the fore limb. Additional responses were then procured from the facial-masticatory muscle complex. The facial area responded with movements of the vibrissae, ear and eye. In view of the precedence in structural development and early use of the forelimbs, and taking into consideration some further significant points, the author concludes that the considerably earlier response from the fore limb area in contrast to the facial area in the opossum, is due to a developmental adjustment and not, as Langworthy (1917a) suggested, to a phylogenetic sequence. No hind limb movements were obtained from any stage of opossum young, nor from a larger series of adult opossums (Weed and Langworthy, 1925; Langworthy, '27a).

Similarly, as in the oppossum, in certain representatives of the placentals the fore limb area has been found to be responsive before the facial and hind limb areas. A series of investigators (Soltmann, 1876; Tarchanoff, '78; Paneth, '85; Langlois, '89; Mills, '94, '95, '96; Michailow, 1910; v. Bechterew, '11; Weed and Langworthy, '26; Langworthy, '27b; and others) have studied the electrical excitability of the cerebral cortex of newborn and very young placentals, using cats, dogs, rabbits and guinea-pigs as experimental animals. The findings of the various authors differ greatly, however.

Mills (1896) found that at birth and for several days after the cortex of cats did not respond to electrical stimulation, but that, in some instances, it was functionally active before the eyes opened, i.e. before the ninth day. The centers for the fore limbs were found to respond earlier than those for the hind limbs, and head movements could be obtained only at a later date than movements of the limbs.

Weed and Langworthy (1926) and Langworthy ('27b), in newborn kittens, which, as in Mills' experiments, were stimulated under ether anesthesia, obtained fore limb movements, while movements of the hind limb could not be obtained until the kittens were 16 days old, while the facial area did not respond to electrical stimulation until the animals were 21 days of age (fig. 7A).

In dogs, Soltmann (1876) found the motor cortex unresponsive until the tenth day after birth, when fore limb movements were first obtained. On the thirteenth day movements of the hind limbs together with fore limb movements could be elicited. But it was not until the sixteenth day that movements were obtained from the facial area. Soltmann narcotized his puppies with ether, chloroform, or morphia.

Later, Ferrier (1880/86) agreed that in dogs no movements could be obtained until after the opening of the eyes, usually about the eighth day; he stated that the cortical centers in the dog generally do not react till about the tenth day, and that the centers for the fore limbs become excitable before those for the hind limbs.

Paneth (1885), however, using no anes-

thesia, obtained positive results in dogs of 24 to 48 hours of age. These puppies responded to electrical stimuli with fore limb movements prevailing, while hind limb movements were sparingly obtained.

Mills (1896) came to the conclusion that there is no proper functional cortical development in dogs before the eyes open, which seldom occurs before the tenth to the thirteenth day. He confirmed Soltmann's results in that he found movements of the head as a whole and of its various parts to be developed later than those of the limbs. In disagreement with Soltmann and with Ferrier, however, he found that in most, though not in all of his specimens, the hind limb area became functionally active at a somewhat earlier date than that of the fore limb, an observation which was not confirmed by subsequent investigators. In the cases of large dogs, such as the St. Bernard, the cortical motor centers became functionally active at a later date than in small breeds, like the terriers, or in mongrels, an important finding that seems to be in harmony with the more rapid physical and psychic development of the latter. Agreeing with Soltmann, Mills emphasized the fact that localization was at first somewhat indefinite, but gradually, though rapidly, became better defined. He found the principal centers active within the first thirty days of life, most of them before this limit. As did his forerunners, he stimulated his dogs while they were anesthetized with ether.

Michailow (1910) and v. Bechterew (1911) likewise obtained positive results in dogs during the first few days of postnatal life. They were then able to locate electrically excitable areas for both fore and hind limbs, for mastication and for neck movements. The facial area became responsive considerably later, not until 5 days after birth. The responses obtained from the facial area were closure of the eyes and ear movements. Several days later they procured, in addition, movements of the upper lip. Von Bechterew, and before him Soltmann, emphasized the fact that their is a considerable individual variability and also variability according to the different breeds of dogs.

Cat and dog, as seen from the experiments of various authors, exhibit the striking phenomenon that the limb areas considerably precede the facial area in their response to electrical stimulation, and that, moreover, the fore limb area precedes the hind limb area. It seems logical to seek the explanation for this latter fact in a developmental adjustment to the functioning of the limbs. For in the placentals, just as in the marsupials, the fore limb plays a greater, guiding rôle from the time that the young of those animals born in immature state first make attempts to walk. But also in the young of those placentals which are well developed at birth, the fore limbs take the leading rôle in progression, just as in the adults of all quadrupedal mammals. This is probably the causal factor for the early cortical motor representation of the fore limbs. In agreement with this are the observations that among the marsupials and primitive placentals the hind limb area in some forms is either absent or only imperfectly represented in the motor cortex of the adult animal. In any case, the hind limbs are far less well represented than the fore limbs, which in all mammals readily respond to electrical stimulation of the cortex. Thus, as regards cortical motor representation, the fore limbs, probably in functional adaptation, precede the hind limbs not only in ontogeny but likewise in phylogeny (see later). It is not astonishing, therefore, that in the dog, as compared with the cat, response from the hind limb and facial areas more quickly follow the response from the fore limb area, from which movements may be obtained at birth in both animals. As is generally known, kittens

are born in a more immature state than are puppies, and the latter make quicker progress in general development during postnatal life. Yet the great delay in the response from the hind limb and facial areas in the cat (Weed and Langworthy, 1926) is rather astonishing. Renewed investigations with modification of the technique (e.g. stimulation of the animals without anesthesia) may yet lessen the apparently great difference between cat and dog.

The results of stimulation experiments on newborn and very young rabbits and guinea-pigs are interesting in comparison with the results which various investigators have obtained in opossum, cat and dog.

In the rabbit Tarchanoff (1878) did not obtain any movements until the animals were about 11 days old. Then it was possible to elicit fore limb and masticatory movements. Only considerably later were movements of the hind limbs obtained. Judging from Tarchanoff's protocols and from his concluding statements, the area for masticatory movements in the young rabbit was more responsive than was the fore limb area, while on the other hand the hind limb area did not give regular response (see later Mills, 1896, on hind limb movements in the rabbit).

In the guinea-pig, however, Tarchanoff found the motor cortex responsive even shortly before birth. This great difference in first appearance of motor response in the guinea-pig compared with the rabbit is undoubtedly correlated with the difference in the state of general development in which these two rodent types are born. While newborn rabbits are even less mature and more helpless than newborn kittens, guinea-pigs are born in a relatively mature condition with a well developed coat of fur and with open eyes, and ready to run around. Von Lenhossék (1889) also emphasized the fact that in the newborn guinea-pig the tracts of the spinal cord, including the corticospinal system, are further advanced in their development and larger in size than in the newborn rabbit or mouse.

In the newborn guinea-pigs Tarchanoff obtained movements of mastication, as well as fore and hind limb movements. Langlois (1889) confirmed these findings. By far the easiest movements to obtain were the masticatory ones, for which Langlois easily located the center. Though he also obtained fore limb movements and, with stronger current, additional movements of the hind limbs, he was unable clearly to localize the limb areas.

Neither Tarchanoff nor Langlois mentioned whether in the masticatory movements of the young rabbits and guinea-pigs facial muscles of the orbicularis oris-buccinator group were involved. The inclusion of movements of this group in mastication is typical for the adult animals, especially in the case of the rabbit. From the fact, however, that Tarchanoff did not specifically state that movements of facial muscles occurred in any of his young rabbits, including those of 45 days of age, it may be assumed that movements of the orbicularis oris-buccinator group really were included in the masticatory movements. If this assumption is correct we have here a case where the fore limb area does not precede the facial area in its development.

It is very likely that in the various types of placentals developmental adjustments of the several motor centers take place according to the differences in behavior during early postnatal life.

III. RESULTS AND CONCLUSIONS FROM ANATOMICAL AND PHYSIOLOGICAL INVESTIGATIONS OF THE MOTOR CORTEX IN VARIOUS ORDERS OF MAMMALS

A. MONOTREMES

Martin (1898/99) explored with great care the entire cortex in two examples of

Ornithorhynchus and obtained practically the same results in each specimen (fig. 1). Movements from the facial field were procured most easily. Stimulation of most parts of the anterior half of the cortex resulted in a "screwing-up" of the contralateral eyelids, but this movement was aroused with the weakest current at a smaller area further back. This "screwing up" of the eyelid is, no doubt, caused through contraction of muscle portions corresponding to the m. orbicularis oculi. This muscle in *Ornithorhynchus*, as well as in *Echidna*, does not form a uniform complete sphincter around the eye, as it does in the marsupials and placentals, but is represented by several muscle portions (see Ruge, 1895; Schulman, 1905) which through synergic action may bring about the same effect as would a uniform m. orbicularis oculi.

From the large, ill-defined responsive motor area on the anterior half of the cortex Martin also obtained contraction of the anterior portion of the opposite "panniculus carnosus," causing the head to be rotated to the opposite side and drawing forward the shoulder. This action was evidently the result of the contraction of that part of the facialis musculature which has been erroneously included in the panniculus carnosus (compare Huber, 1931b). Responsible for the movement referred to by Martin is a powerful muscle of the superficial facial musculature (m. platysma), which expands from the shoulder region over the lateral side of the face to be secondarily attached to the duck bill shaped snout (see Huber, 1931b). Owing to its attachment this muscle may rotate the head and in addition may exert a pull upon the shoulder, just as Martin observed in his stimulation experiments. From these observations it becomes evident that the facialis field is well represented in the motor area of the cerebral cortex of *Ornithorhynchus*.

In addition, upon stronger stimulation, Martin obtained from the same large, ill-defined area on the anterior half of the cortex, definite advancement of the shoulder with flexion of the elbow and extension of the wrist followed by abduction of the shoulder, extension of the elbow and flexion of the wrist. The movement was, as Martin ascertains, precisely that exhibited by the fore limb of the animal when digging a burrow.

Martin's experiments reveal the important fact that in *Ornithorhynchus* facial and fore limb areas partly coincide. It is important, moreover, that Martin, even with the strongest current, failed to obtain the slightest movement of the hind limbs or tail from any part of the cortex.

B. MARSUPIALS

In the marsupials, especially in the opossum (*Didelphys*), the motor facial area has been reported by most authors as partly coinciding with areas from which movements of the fore limbs were also obtained (fig. 2). The fore limbs, on the other hand, appear still to have partial representation in the corpus striatum (Rogers, 1924).

As to the localization of the hind limbs in the opossum, the various authors do not agree in their findings. Some of them claim to have been able to localize a hind limb area in the motor cortex. Thus Herrick and Tight (1890), investigating the opossum, reported hind limb movements from an ill-defined area lying posterior to the orbital sulcus. Ziehen (1897) also reported that he obtained hind limb movements in the opossum, in addition to movements from fore limb and facial areas, which in part overlapped. The center for the facialis musculature required less current than that for the fore limb, which in turn responded to less current than the center for the hind limb, from which movements were obtained only with a very strong current.

Flashman (1906) reported hind limb movements in *Dasyurus*, which were, however, inconstant and diffuse.

C. and O. Vogt (1906/07) observed hind limbs movements in stimulating various representatives of the di- and polyprotodont marsupials. They emphasized, however, that these movements were very difficult to obtain, and were diffuse and inconstant—in fact, in several kangaroos no hind limb area could be detected—while the fore limb area in the di- and polyprotodont marsupials investigated always responded to the stimuli. The hind limb movements, when obtained, were usually movements of the whole leg, though occasionally only the musculature of the foot was involved. In contrast to this, discrimination of finer movements in the fore limb, as in the facial region, was quite marked.

Cunningham's (1897/98) reference to hind limb movements in one of his experimental animals (opossum) is not to be counted among the positive results. As Cunningham himself states, the animal was coming out of ether when these movements occurred. This made him conclude that they were of the spontaneous kind.

According to the observations of all these investigators, the hind limb area thus strikingly contrasts with the fore limb and the facial areas in its response to electrical stimulation. One may suspect that some of the investigators obtained hind limb movements through application of unduly strong current. It is most significant that several later investigators (Rogers, 1923, '24; Gray and Turner, '24; Turner, '24; Weed and Langworthy, '25; Langworthy, '27a) were unable to obtain movements of the hind limbs, trunk or tail in the opossum, despite careful exploration of the whole cerebral cortex.

This seems to be in agreement with the observation of Turner (1924) that in the opossum which he investigated the corticospinal tracts did not extend caudalward beyond the cervical region. Turner, however, admitted that these tracts might possibly continue downward for some distance as unmyelinated fibers which do not constitute a compact tract, but are too scattered to be defined by the Cajal and Ranson silver stains.

Nevertheless, we can not completely disregard all the data of the former group of investigators of marsupials in view of the fact that among these authors who claimed positive, though inconstant results, were C. and O. Vogt, who had much experience in cortical stimulation. It is tempting to assume that in the evolution of their motor cortex the marsupials have not reached the stage where the motor centers for the hind limbs have definitely shifted into the cortex, and that their occasional, though ill-defined response depends on progressive individual variations of the experimental animal, besides other specially favorable conditions. Such a hypothesis would reconcile the conflicting results obtained by the various workers. In such instances where response of the hind limbs is obtained, subsequent investigation of the pyramidal system should be undertaken in order to ascertain whether the corticospinal tracts really extend caudalward as far as the lumbar or lumbosacral level of the spinal cord.

Within the facial area of the opossum, movements of the ear musculature, of the m. orbicularis oculi, and of the vibrissae can be elicited. The muscle group that moves the facial vibrissae is well represented in accordance with the important functional rôle that this tactile mechanism plays in the life of the animal.

C. PLACENTALS

While we have fair, though by no means fully correlated and exhaustive knowledge of the motor cortex of the more common

laboratory placental mammals (rat, rabbit, cat, dog, monkey (*Macacus* and *Cercopithecus*)) and of some of the rarer experimental forms such as the orang-utan, chimpanzee and gorilla, our knowledge of other placentals is still very fragmentary. There are a series of orders and suborders which have not been subjected to experimentation, while of the others only one or a few representatives have been studied.

1. *Insectivores*

Of this order only the hedgehog (*Erinaceus europaeus*) has been investigated (Mann, 1896; Ziehen, '97, '99; Probst, 1901; Weber, '06; C. and O. Vogt, '06/07) (fig. 3). The results obtained by these authors differ widely. They are in part uncertain and incomplete. For example, Mann failed to obtain response from any part of the facial musculature, while Ziehen (1897) and C. and O. Vogt (1906/07) observed movements of neither the m. orbicularis oculi nor of the ear musculature. Evidently those facial muscles which during the life of the insectivores play the greatest rôle, (i.e. the muscles of the snout) are the best represented (Ziehen, Probst, C. and O. Vogt), while the other muscles of the facialis group do not readily respond (see Ziehen '99, p. 169, in contrast to '97). The snout musculature appears to be in part bilaterally represented (C. and O. Vogt).

In addition to the facial area, centers for the fore and also for the hind limbs have been located by Mann, Ziehen and C. and O. Vogt. The responses obtained involved isolated, uncorrelated movements of only certain muscle groups. According to Ziehen, the hind limb area requires stronger stimuli than does the fore limb area. The Vogts emphasized the fact that the motor areas of the hedgehog in general are decidedly less responsive than are those of higher placentals. This may be the explanation for the meagre or fully negative results of Probst and Weber.

The experimental findings of the various investigators seem to be in accord with the fact that in the hedgehog the cyto-architecture of the motor cortex is as yet but little evolved (Brodmann, 1909).

2. *Bats*

One representative of the Microchiroptera, evidently *Vespertilio murinus*, the common small bat of central Europe, has been subjected to experimentation by Ziehen (1899) and by Merzbacher (1903). The results are not worth mentioning.

C. and O. Vogt (1906/07) stimulated eight specimens of *Pteropus Edwardsi* (fruit bat), a representative of the Macrochiroptera. They found the "head segment" with the facial area to be far more extensive and more responsive than the leg areas (fig. 4). Various subdivisions of the facial area were mapped out. These responded with three different kinds of "rhythmic movements" of the muscles of the snout, m. orbicularis oculi and ear musculature. From the leg areas the authors obtained what they considered satisfactory results only in their last specimen. These findings differed widely from those obtained from the other seven specimens. Both fore and hind limbs were represented, with partial overlap of the areas. These experiments have not been repeated.

3. *Rodents*

A series of authors have investigated, both morphologically and experimentally, the motor cortex of mouse, rat, rabbit, guinea-pig and ground hog (*Arctomys monax*).

C. J. Herrick (1926, p. 164) sums up with the following statement the work of Döllken, 1907; Brodmann, '09; Isenschmid, '11; DeVries, '11; Winkler and Potter, '11; and Fortuyn, '14; who have independently

charted the cortical areas of rabbit, mouse and rat: "When the reports of their examination of the same species are compared the result is disquieting. All agree that in these lowly forms the boundaries between the various areas are indefinite and that the structural features by which the areas are distinguished are far less well defined than in higher brains."

In accord with this are the results obtained from cortex stimulation and ablation experiments by Ferrier, 1873, '74, '80/86; Fürstner, '76; v. Bechterew, 1886/87, '87, '11; Exner and Paneth, '87; Herrick and Tight, '90; Mann, '96; Mills, '95, '96; Ziehen, '97; Lashley, 1921; and W. K. Smith (unpublished). Upon considering this literature we realize that the data so far obtained are very incomplete, in part contradictory, and little correlated.

The excitable cortical centres of the rodents appear to be ill defined and variable. Several workers were unable to obtain hind limb movements, while others claimed a responsive hind limb area in addition to the areas for the fore limb, facial and masticatory musculature, etc. In these studies are included uncritical references to simultaneous movements of both hind limbs (Mann).

Mills (1896), after examination of a large series of different breeds of young and adult rabbits, emphatically denied the existence of a responsive hind limb area in this animal. He thus found the rabbit to be in striking contrast to the guinea-pig, rat and mouse. In these latter rodents he was able to confirm the general motor localization as previously found by Ferrier, who located a hind limb area in addition to the other areas. It is most significant that in the rabbit (compare v. Lenhossék, 1889; Simpson, 1914, '15), as in a series of other rodents (compare v. Lenhossék, 1889; Goldstein, 1903/04; Wallenberg, '03/04; v. d. Vloet, '06; King, '10; Simpson, '12/13, '14, '15; Ranson, '13, '14; Linowiecki, '14; Reveley, '15), the corticospinal tract can be traced, although greatly reduced, at least as far as the lumbar portion of the spinal cord. In some cases this tract extended even to the sacral region. Thus in these rodents, including the rabbit, there evidently exists a definite anatomical basis for positive cortical motor response of the hind limbs. Swank (1932), however, consequent to extensive cortical destruction, could not trace the corticospinal tract of the rabbit below the upper level of the first cervical segment.

A decided advance was made through the fundamental studies of Lashley (1921), who combined cortex stimulation and ablation experiments with studies on behavior (fig. 5). He found that destruction of the electrically excitable cortex in the rat produced no discernible motor disturbances, but that simultaneous destruction of the motor area and the corpus striatum resulted in paralysis which resembled the effect of destruction of the excitable cortical area in more highly organized brains. Using these results as a basis, Lashley concluded that in these lowly placentals the motor cortex has a regulating function rather than real control over the voluntary movements. This seems to be in agreement with what Rogers (1924) found in a marsupial, the opossum.

From the available data on insectivores, bats and rodents, it becomes clear that in regard to the differentiation of the motor cortex these lowly organized placentals have evolved but slightly above the marsupials, and that in the primitive mammals (marsupials and primitive placentals), as Herrick (1926, p. 165) states, "the cortical differentiation is still in its incipiency."

4. Ungulates

A few investigators (Marcacci, 1877; Ziehen, '99; Dexler and Margulies, 1906;

Simpson and King, '11; Bianchi, '20; Bagley, '22) have studied the motor cortex in this group, using the sheep as their experimental animal (fig. 6). The results are in part contradictory and indefinite. A series of responsive areas which show considerable individual differences have been mapped out. Among these is a facial area, which gives response of the contralateral and in part bilateral facialis musculature. Besides the main facial area Bagley located an additional well defined area which readily responded with ipsilateral contractions of facial muscles of the mouth. In agreement with Marcacci and in contrast to Simpson and King, Bagley found the facial area more responsive than the other areas. According to most authors (Marcacci, Ziehen, Bianchi, Simpson and King) movements of the fore legs are contralateral, and response from them is more easily obtained than from the hind legs. Marcacci was unable to obtain movements from the latter. In connection with this, it is important to note that Bagley was unable to trace the corticospinal tract beyond the cervical region.

The several authors emphasize the great difficulty of experimentation in the sheep. Dexler and Margulies stated that the results which they obtained were inconstant and uncertain. Simpson and King mentioned that in many individuals, motor responses were not obtained from one or more of the areas. And Bagley agreed with them, stating that only through frequent attempts were results obtained. Nothing could better illustrate how indefinite these results are than Bagley's statement (1922, p. 22) that upon stimulation in the leg areas he often obtained response from 2, 3 or even all 4 legs. These results must be carefully weighed in view of the fact that Bagley used only local anesthesia. Likewise Simpson and King, in part stimulating animals in the conscious state, obtained, at times, movements of the opposite fore and hind legs together with movements of the hind leg of the same side. Moreover, they obtained from the left cortex of a 6 week old lamb, sucking movements combined with "wriggling" movements of the tail, upon stimulation of an area which corresponds to the facial area of the adult sheep. Under ether anesthesia this "sucking center" could not be found on the right side, nor was this area excitable on either side in a second lamb which was subjected to stimulation.

It should be added that destruction of the motor cortex in the sheep caused no obvious paralysis (Simpson and King).

In spite of the partly indefinite results and questionable data of these studies on the sheep, we may conclude that the motor cortex in this ungulate has evolved scarcely further than it has in the above discussed lower placentals.

5. Carnivores

A series of authors (Fritsch and Hitzig, 1870; Ferrier, '73, '74, '80/86; Hitzig, '73, '74, '04; Luciani and Tamburini, '78; Rosenbach (and v. Bechterew), '83; Paneth, '85; v. Bechterew, '86/87, '87, 1911; Exner and Paneth, '87; Herrick and Tight, '90; Mann, '96; Mills, '94, '95, '96; Weber, 1906a; Weed and Langworthy, '26; Langworthy, '27b; Smith,' 32; and others) have investigated the motor cortex in carnivores, especially in cat and dog. From these investigations and from our own on the cat and dog (Huber and Smith, unpublished), it becomes evident that the motor cortex in these carnivores has evolved further than in representatives of the above discussed orders of placentals.

The electrically excitable areas are situated adjacent to the cruciate sulcus (see fig. 7B). Referring in this discussion only to the areas for the limbs and face, we may note that the motor cortex responds

with more elaborate movements of the entire musculature of the fore and hind limbs (see Smith, 1932, in regard to the greater development of the fore limb area compared to the hind limb area in the American black bear).

The readily responsive facial area appears to be clearly separated from that of the fore limb. Contraction of the facialis musculature is prevailingly contralateral, but to a certain extent also bilateral. Within the facial area those muscles that move the long mystacial tactile vibrissae (m. naso-labialis and m. maxillo-naso-labialis) are the most responsive ones, especially in the cat. Portions of the m. orbicularis oculi contract synergically with the m. naso-labialis. The two muscles constitute a morphological and functional unit, and the cortical center for this genetic muscle group evidently has not undergone a further clear subdivision.

From the well defined leg areas purposeful movements of the contralateral limbs are regularly and easily obtained, this being in contrast to the placentals already discussed. There is a small transitional zone between the fore limb and hind limb areas from which movements of the hind limbs are initiated simultaneously with the last slight movements of the fore limb. The movements of the limbs are those which occur in the characteristic progression of the animal. This can be most clearly demonstrated in the case of the dog (Huber and Smith).

None of the more primitive carnivore types have so far been investigated satisfactorily (compare e. g. Weber, 1906a).

6. *Primates*

Within the primates a far greater variety of forms has been studied than in any other order of the placentals. It is indeed of great importance that these investigations should cover the main groups of the primates so that, on the one hand, the findings on the lower representatives of this order might be linked with those on primitive types of other placental orders, and, on the other hand, through comparative study of the catarrhine monkeys and of the anthropoid apes, a key to the understanding of the complex conditions found in the human motor cortex might be obtained.

It should be added that *the author has purposely confined himself to the discussion of the true "motor" cortex.* He has refrained from reference to the excitable occular areas on the frontal and occipital poles of the fore brain, which give responses of an entirely different character, not to be confused with the typical response of the main motor area, a fact which for the first time was clearly pointed out by Grünbaum and Sherrington (1901/02).

A. Prosimiae (Tarsioidea and Lemuroidea)

Tarsius has not yet been subjected to cortical stimulation, but there are records of observations on a number of the Lemuroidea.

Völsch, in collaboration with Ziehen, published (1906) a short account of the stimulation of one lemur, probably *Lemur mongoz*. The data obtained were rather incomplete.

This study is preceded by a short reference to the stimulation and histological investigation of the brain of *Perodicticus* by O. Vogt (1906), in connection with a preliminary communication of extensive studies by C. and O. Vogt on the motor cortex.

Soon afterwards appeared the fundamental work of C. and O. Vogt (1906/07), containing the results of stimulation experiments on eight Lemuroidea of various genera and species: 3 *Lemur macaco*, 2 *L. mongoz*, 1 *Perodicticus potto*, 1 *Chirogale crossleyi* and 1 *Otolemur* (= *Galago*) *crassicaudatus*. This study was correlated with histological investigations by Brodmann (compare C.

and O. Vogt, 1906/07, p. 407; Brodmann, 1906).

Then followed the publication by Mott and Halliburton (1908) of stimulation and ablation experiments on one lemur, correlated with histological studies.

From the studies of these various authors it may be concluded that the motor cortex of the brain of the Lemuroidea has not attained a high degree of differentiation. Location, extension and subdivisions of the motor cortex were found similar to those in the lower platyrrhine monkeys.

In *Perodicticus* (fig. 9) the responsive motor area showed a relation to the sulcus "ce" similar to the relation of the motor area to the sulcus centralis in the simian brain. This was corroborated through histological studies (O. Vogt, 1906; Brodmann, 1906). The Vogts therefore declared these sulci homologous. Brodmann, moreover, gave evidence that the sulcus "e" of the brain of *Lemur*, *Nycticebus* and *Propithecus* is not identical with the sulcus centralis. He demonstrated that sulcus "e" lies amid the area precentralis, which is characterized by large pyramidal cells, the motor cells of Betz (1874) (compare Brodmann, 1906, p. 377; 1909/25, p. 168/169, 175 and fig. 100). These data suggest great caution in attempts to homologize the cortical sulci of the various, diversified types of the lemuroid stock.

In the brain of the Lemuroidea distinct responsive areas for the head, trunk, tail and both extremities have been mapped out (see figs. 8 and 9). The sequence of parts represented, proceeding from the superior surface of the hemisphere downward, is: Tail, hind limb, trunk, fore limb, head. This arrangement is characteristic of all primates. From the facial area movements of the m. orbicularis oculi and the musculature of the ears and snout have been obtained. None of the investigators referred to vibrissae movements, which in the lemur should be expected to be an outstanding phenomenon. Easily obtainable contralateral ear movements were reported (C. and O. Vogt, Mott and Halliburton). This is in agreement with the fact that in the lemurs the ear musculature is well developed and differentiated (Ruge, Huber), corresponding to the great rôle which it plays in the prevailing nocturnal life of the animals. The closure of the eyes was found to be prevailingly contralateral, while the snout musculature gave both contralateral and bilateral responses (C. and O. Vogt). The Vogts emphasized the more caudal position of the area for closure of the eyes as being in striking contrast to the more oral position of the same area in the simian brain.

From various motor areas of the lemur brain responses were evidently difficult to obtain and not constant throughout, as can be seen from the protocols of the Vogts, and from the fact that Ziehen and Völsch only with stronger current obtained a single movement of the extensor musculature of the thigh, while no movements could be obtained from the leg, feet or toes.

B. Simiae

I. Platyrrhine monkeys

The extensive investigations of C. and O. Vogt (1906/07) included experiments on 21 specimens, representatives of both families of the platyrrhines.

a) Family Hapalidae (= Callitrichidae) (marmosets)

The animals subjected to experimentation were 4 *Callithrix (= Hapale) penicillatus*, 2 *C. jacchus* and 1 *Midas ursulus (= Mystax midas)*.

In agreement with the lowly position of the marmosets their little or unfurrowed (lissencephalic) brain has no sulcus cen-

tralis. The responsive motor cortex, however, shows a topographic relation similar to that found in the representatives of the Cebidae, the second family of the platyrrhines, which possess a well defined sulcus centralis (see fig. 10) or, in the case of *Nyctipithecus* (= *Aotus*), at least indications of a central furrow (compare C. and O. Vogt, p. 390, and figures on plate 7). This fact, which is in full agreement with the histological findings of Brodmann (1909/25), is very important. It proves that in the phylogeny of the primate brain the cyto-architecture precedes the formation of the sulci and gyri.

In the motor cortex of the marmosets responsive areas for the trunk, tail, upper and lower extremities and head, including the facial field, have been mapped out. Stimulation of the arm area gave prevailingly generalized arm movements. Finger movements, when obtained, almost always involved all of the digits. The authors emphasized the extreme difficulty in obtaining any isolated finger movements. This seems to be a further instance of the lowly organization of the marmoset brain compared with those of the higher platyrrhines and the catarrhines.

In an additional study on a marmoset, Mott, Schuster and Halliburton (1910) reported the results of stimulation and extirpation experiments combined with histological studies. The authors emphasized the large extension of the head area, including the facial field, which could be further subdivided and gave a more ready response than the fore and hind leg areas. In contrast to their previous investigation on the lemur (Mott and Halliburton, 1908), no response from the ear musculature was obtained in the marmoset. Nor did the Vogts record ear movements. However, this negative result must not be considered final. It should rather be expected that a more elaborate investigation of the facial area will yield ear movements, since the marmosets possess a well developed and fully functioning, though not highly differentiated ear musculature (Huber, 1931a).

b) Family Cebidae

C. and O. Vogt seem to be the only authors who have experimented on representatives of this family. They investigated 2 *Nyctipithecus* (= *Aotus*) *trivirgatus*, 1 *Pithecia satanas*, 1 *P. hirsuta* (= *P. monacha*), 5 specimens of *Cebus*, including various species, 1 *Alouatta nigra* (= *Alouatta caraya*), 1 *A. seniculus* and 3 *Ateles ater*.

Nyctipithecus (= *Aotus*) is of special interest, as this type, in regard to many structures, including the facialis musculature (Schreiber, 1928; Huber, 1931a), appears to combine characteristics of the lowly marmosets and the higher Cebidae. Of the two specimens examined by the Vogts, one, in correspondence with the marmosets, showed no indication of a gross morphological demarcation between the electrically responsive precentral and non-responsive postcentral regions, while in the other specimen these areas were separated by a furrow, evidently homologous to the well marked sulcus centralis of the higher Cebidae and all the catarrhines, including the Old World monkeys, apes and man. The topography and subdivisions of the responsive motor areas of *Nyctipithecus* were found to be largely the same as in the marmosets, with fewer absolute foci than in the higher Cebidae (pp. 386–387 and plate 7).

In all the higher Cebidae the topography of the motor cortex was found similar to that of the lower catarrhines (fig. 11). The responsive area never extended posterior to the sulcus centralis Rolandi. Some important differences were noticed in the various types. Those concerning the tail area are significant. It is not

astonishing that this area in the higher Cebidae has undergone a greater extension and fuller elaboration. The higher representatives of the Cebidae, in contrast to the marmosets, the lower Cebidae and all of the catarrhines, possess a prehensile tail which is a great aid in the arboreal activity of these monkeys. This tail is used much as a fifth hand. Through acquisition of high sensitivity on the hair free flexor surface, this prehensile tail has become a remarkable sensory organ, which shows the highest differentiation in the woolly monkey (*Lagothrix*) and in the spider monkey (*Ateles*) (see further Tilney, 1928, vol. I, p. 192). Thus it is not astonishing that the tail area in the spider monkey (*Ateles*) was found to be more extensive and better isolated than in *Alouatta*, *Cebus* and *Pithecia*. This area was found further subdivided, and it gave a more ready response than in the other Cebidae. In the spider monkey more elaborate movements of the hand and fingers were also obtained. These interesting data suggest indeed a close correlation of morphological and physiological features of the motor area with the habits of the spider monkey, the cleverest acrobat among the platyrrhines, an arboreal type similar to the gibbon among the catarrhines.

As to the facial area of the platyrrhine cortex, C. and O. Vogt's investigations bring only scanty contributions. It should be expected that here also a further elaboration would take place in the ascending scale of platyrrhines, paralleling the evolution of the facialis musculature. In the higher platyrrhines the fuller use of the more elaborate muscles of the face proper (mimetic muscles) is in striking contrast with the prevailing use of the musculature of the ear and snout and the specialized vibrissae-moving facial muscles in the lower platyrrhines and prosimians (see Huber, 1931a).

II. Catarrhines

a) Catarrhine Monkeys (Cercopithecidae sive Lasiopygidae)

Hitzig (1874, '04) was largely correct with the exploration and delimitation of the motor cortex of his only macaque (*Macacus* sive *Pithecus*). Great credit must be given to him, because he was the first clearly to demonstrate and emphasize the fact that the electrically responsive area does not extend behind the sulcus centralis Rolandi.

Following the publication of Hitzig, Ferrier (1874, '75, '80/86) claimed a much more extensive motor area for the macaque, including the postcentral gyrus, with additional areas scattered all over the outer surface of the cortex and even an area on the lower surface of the temporal lobe (gyrus uncinatus). This great deviation between the findings of Hitzig and Ferrier was obviously due to the application of too strong a current by the latter. Ferrier's detailed exploration of the motor area in a series of macaques brought further valuable data. Careful examination of the detailed protocols, however, reveals a series of very questionable and uncritical observations.

Ferrier's ablation experiments undertaken on several macaques were an important addition. With these experiments he demonstrated that destruction of the whole or part of the excitable motor area results in disturbances more serious than in the dog, cat and rabbit. These latter had been subjected to similar experiments by a series of investigators (Hitzig, Ferrier, Munk *et alii*). It had become known that in dog and cat the effects of destruction of the motor centers, though in the beginning well marked, largely disappeared within a few days. In the rabbit such disturbances were found even less severe than in the cat and dog, and also of much

shorter duration. Since Ferrier was unable to keep alive his operated macaques over a sufficiently long period, he was unable to observe whether the marked paralysis was temporary or permanent. Subsequent ablation experiments by other investigators (see e.g. Biedl, 1897) showed that in the macaque restitution of the impaired functions takes place (see also Fulton and Keller, 1932). The gravest and longest lasting paralyses appear to be those of the hand.

A series of investigators (Horsley and Schäfer, 1884, '89; Schäfer, '87; Beevor and Horsley, '87, '94; Munk, '90; Flood, '94; v. Bechterew, '98, '99; and others) subsequently investigated the motor cortex of lower catarrhines, especially of the Bengal macaque, *Macacus* (= *Pithecus*) *rhesus*, and the guenon, *Cercopithecus callithrix* (= *Lasiopyga callitrichus*). Partly influenced by Ferrier's inaccurate findings, these authors mapped out the motor area in an extension far greater than that which actually exists. They always included the postcentral gyrus in the electrically responsive motor area. Although later investigators have proven this to be incorrect, these studies nevertheless contributed greatly to the knowledge of the motor cortex of the lower catarrhines.

Grünbaum and Sherrington's convincing demonstration (1901/02) that in the anthropoid apes the excitable cortex is restricted to areas anterior to the sulcus centralis Rolandi should have led to a reconsideration of Hitzig's previous similar restriction of the motor cortex in the macaque. Yet further studies upheld Ferrier's view.

Munk (1903), in his investigations of the changes in motility following loss of sensibility in the upper extremity in fourteen Old World monkeys (*Macacus* (= *Pithecus*) *rhesus*, *M. cynomolgus* (= *Pithecus irus*), and *Cynocephalus* (= *Papio*)), mentioned the fact that he regularly obtained, with weak current, flexion of the thumb and in addition (if his statement on pp. 1072–1073 is rightly interpreted) closure of the hand and movements of the fore arm from various points of the postcentral gyrus, which he compared with corresponding sections of the precentral gyrus. He stressed the fact that he stimulated his monkeys in the conscious state, which enabled him to appreciate finer differences in motility response. Thus he found, in contrast with previous workers, that loss of sensibility in the extremities resulted in a notable decrease of the motility.

Rothmann (1904) claimed that in various representatives of macaques which he had studied the postcentral gyrus was directly excitable. Because of this he insisted that the postcentral gyrus is to be included in the motor field. The evidence most convincing to him lay in the fact that three weeks after ablation of the precentral area combined with partial section of the lateral cord, definite response was obtained from the corresponding postcentral segment upon stimulation. It should be noted that he used a very strong current. In a second paper (1907) Rothmann defended anew his viewpoint against the opposite conclusions of earlier workers (Hitzig, 1874, on the macaque; Grünbaum and Sherrington, 1901/02 and '04, on the anthropoid apes; and Brodmann, '04, on a series of lower catarrhines, namely macaques and guenons) who claimed that the postcentral gyrus was electrically inexcitable.

Weber (1906), after stimulating a macaque, again confirmed Ferrier's findings, including the extension of the excitable motor area into the postcentral gyrus.

Then followed the important contributions of various investigators, which finally led to a more correct delimitation of the motor areas in the lower catarrhines. The

findings of these authors confirmed Hitzig's (1874) restriction of the electrically excitable motor cortex to areas anterior to the sulcus centralis Rolandi.

Probst (1903) emphasized the fact that in stimulating a series of pig-tailed macaques, *Macacus* (= *Pithecus*) *nemestrinus*, and hamadryas baboons, *Cynocephalus* (= *Papio*) *hamadryas*, he always found the precentral convolution excitable in its entire extent. From the postcentral gyrus, on the other hand, motor responses, if obtained at all, were elicited from single points only. In several of Probst's animals the postcentral gyrus gave no response whatever. He therefore considered the motor function of the postcentral gyrus to be of subordinate importance, yet admitting that this area has a small share in the motor field of the cortex.

More radical, however, was Brodmann (1904), who opposed Rothmann in a vigorous discussion, referring to Grünbaum and Sherrington's investigations upon anthropoid apes and to the results of the then recent stimulations of the motor cortex of human patients by Krause (1904) which he personally had witnessed. Brodmann was in the position to present direct evidence from stimulation and ablation experiments, which he had undertaken jointly with C. and O. Vogt, that also in the lower catarrhines (macaque and guenon) the excitable motor area is confined to the precentral gyrus (see further Brodmann, 1905).

Jolly and Simpson (1907), inspired by Grünbaum and Sherrington's masterly experiments on anthropoid apes (1901/02, '04) and in doubt about Rothmann's findings, undertook a more careful investigation of the motor cortex in *Macacus* (= *Pithecus*) and *Cercopithecus callithrix* (= *Lasiopyga callitrichus*). After inserting a thin plate of vulcanite into the sulcus centralis they stimulated the postcentral gyrus, and obtained no response. By so modifying their technique they now limited the motor cortex to areas anterior to the sulcus centralis. The studies of following authors, including ourselves (Huber and Smith), bring evidence that such special care of isolation is not needed, and that careful technique and discriminative judgment lead to the same correct results.

About the time of Jolly and Simpson's publication, Sherrington, in his book on "The Integrative Action of the Nervous System" (1906), emphasized the fact that in *Cercopithecus callithrix* (= *Lasiopyga callitrichus*) the excitable motor area does not extend posterior to the sulcus centralis (Sherrington, 1906, fig. 75, p. 297). The same was demonstrated in the study by Roaf and Sherrington (1906) on *Cercopithecus callithrix* (= *Lasiopyga callitrichus*) and *Cynocephalus anubis* (= *Papio* sp?).

Also about this time the work of C. and O. Vogt (1906/07; see also preliminary report by O. Vogt, 1906) appeared. These authors experimented on a variety of animals of the subfamily Cercopithecinae (= Lasiopyginae): *Macacus* (= *Pithecus*) *rhesus*, *Inuus inuus* (probably the Barbary ape, *Simia sylvanus*), *Cynomolgus* (= *Pithecus*) *fascicularis*, *Cynomolgus* (= *Pithecus*) *sinicus*, *Cercocebus fuliginosus* (= *Cercocebus aethiops*), *Cercocebus collaris* (= *Cercocebus aethiops*), *Mona* (= *Lasiopyga*) *mona*, *Mona* (= *Lasiopyga*) *campbelli*, *Cercopithecus pygerythrus* (= *Lasiopyga pygerythra*). The data obtained from the experiments on these monkeys, 27 in number, were discussed in detail and charted in both individual and combined schematic figures (for latter, see C. and O. Vogt, plate 3, figs. 2a and b, 3). In addition, a single specimen of *Semnopithecus* (= *Pygathrix*) *entellus*, representing the Semnopithecinae (= Colobinae), the second sub-family of the Cercopithecidae, was stimulated. The results obtained from this animal (fig. 13) were practically

identical with those procured from the Cercopithecinae.

In the catarrhine monkeys, as in the platyrrhines, occasional reactions may be obtained from points of the postcentral area close to the sulcus centralis, if somewhat stronger current be applied than that necessary to elicit response from the corresponding sections of the precentral gyrus. This is a generally observed phenomenon and was probably responsible for the misconception that the excitable motor cortex extends into the postcentral gyrus. The excitability in the postcentral gyrus gradually fades out caudalward in the direction away from the sulcus centralis, and no genuine response from the postcentral gyrus can be obtained. The Vogts gave the explanation that the apparent response from the area posterior to the sulcus centralis must be due to irradiation of the current from there to the corresponding section anterior to the sulcus. This they found to be in accord with the fact that after ablation of the postcentral area there was no paralysis in the organs which became paralyzed through destruction of the corresponding segment of the precentral gyrus. Thus the Vogts definitely restricted the excitable motor area in all the lower catarrhines, just as in the platyrrhines, to the area anterior to the sulcus centralis (see figs. 12 and 13).

In regard to the responses from the precentral gyrus of the lower catarrhines the findings of the Vogts largely agree with those of both earlier and later authors, including ourselves (Huber and Smith) on the macaque. Response is readily and consistently obtained and the movements elicited are quite elaborate, though not as elaborate as those found in the anthropoid apes. Thus, for example, stimulation of the hand and finger area produces no such clearly isolated movements of the individual fingers as Sherrington and his coworkers have obtained in the anthropoid apes. Likewise the movements obtained from other parts of the body are not as detailed as in the anthropoid apes (see later).

Lewandowsky and Simons (1909), being still in doubt about the inexcitability of the postcentral gyrus, and rather favoring Rothmann's findings, undertook investigations with a view to settling this question. Repeating the experiments of Brodmann (1905), they extirpated the precentral gyrus in a series of eight Old World monkeys (macaques and guenons), leaving the postcentral gyrus intact for subsequent stimulation. After the elapse of a long period (3 to 6 weeks), they found that following this destruction of the precentral region the postcentral gyrus was not responsive to stimulation by even an extremely strong current. Extirpation of the postcentral gyrus, on the other hand, did not influence the excitability of the precentral convolution, which, as before, remained responsive to weak currents.

Convincing as were these experiments, Rothmann (1912) nevertheless opposed Lewandowsky and Simons, producing data from additional investigations. Yet the results of temporary freezing of parts of the motor cortex, an ingenious method introduced by Trendelenberg (1910, '11) in an attempt to avoid possible accessory injuries with the ablation method, did not produce satisfactory confirmation, as Rothmann himself admitted. Nor are the results of Rothmann's additional and somewhat modified ablation experiments convincing.

Lewandowsky and Simons (1913) justly criticized Rothmann (1912), in that he failed to verify the extent of his ablations by histological study, which method alone would have given full proof as to whether or not he had succeeded in completely removing the precentral gyrus.

Fulton and Keller (1932) investigated the effects of ablation of the cortical leg area upon the plantar reflexes, especially the Babinski, in a series of catarrhine monkeys of various types. They found that removal of the leg area on one or both sides of the motor cortex failed to produce the Babinski reflex in the macaque (*Pithecus*), mangabey (*Cercocebus*), patas monkey (*Erythrocebus*), and guenon (*Lasiopyga*). Only when the lower lumbar segments were freed completely from higher control by hemisection of the spinal cord was the Babinski response noted in these monkeys. This indicated that the Babinski in these animals is not controlled by the corticospinal pathways. In the baboon (*Papio* and *Theropithecus*), on the other hand, removal of the cortical leg area led to the appearance of the Babinski reflex. The significant observation was made that the baboon was slower to recover motor power following a lesion in the leg area than were the other monkeys. Fulton and Keller therefore concluded that "cortical dominance" is more highly developed in the baboon than in the other Old World monkeys studied.

In addition to the above quoted stimulation and ablation experiments, detailed investigations on the cyto-architecture of the motor cortex of the brain of lower catarrhines (*Macacus*, *Cerocopithecus*) were undertaken by various authors (Mellus, 1904/05; Brodmann, 1906; Nanagas, 1922/23; and others). The results of these studies agree closely with those obtained from electrical stimulation.

Thus gradually, through the contributions of a number of investigators, a more accurate and fuller knowledge of the so-called "motor" cortex of the lower catarrhines has come about.

b) Hylobatidae (Hylobates and Symphalangus)

The work of C. and O. Vogt (1906/07) includes a short reference to a stimulation experiment by Brodmann on the left hemisphere of a siamang (*Symphalangus syndactylus*) (fig. 15). It would be expected that the Hylobatidae would occupy a position intermediate between the lower catarrhines and the big anthropoid apes. This was found to be true. In contrast to the lower catarrhines the head area in this siamang appeared relatively small in comparison with the extremity areas. Among these the arm area exhibited a striking increase in size.

A fuller experimental and histological investigation of the motor cortex in the "black" gibbon (= *Hylobates* sp ?) was later undertaken by Mott, Schuster and Sherrington (1911) (fig. 14). The authors emphasized the remarkable forward expansion of the responsive motor cortex (fore limb area) in the region of the middle frontal convolution. This was explained as a correlation with the use of the arms in the brachiating habits of the gibbon.

Fulton and Keller (1932) obtained essentially similar results from cortical stimulation of a specimen of *Hylobates agilis*. They also extirpated the leg area in this specimen (as well as in an example of *Hylobates leucogenys*). This led to the appearance of a strong positive Babinski reflex; and the return of voluntary power was much slower than in either the baboon or the other catarrhine monkeys that they studied. These results indicated the greater development of "cortical dominance" in the gibbon.

c) Pongidae or Great Anthropoid Apes (Orang-utan, Chimpanzee and Gorilla)

It was not until 1891 that for the first time the motor cortex of a representative of the great anthropoid apes, an orang, was subjected to experimentation by Beevor and Horsley. This was a further step in the study of the primate cortex and toward the knowledge of the motor functions of

the human brain. A comparison of this publication with Beevor and Horsley's previous study on the macaque (1887) reveals a far more exact delimitation of the responsive motor area, which however, was still shown as extending partly into the postcentral gyrus. A series of details, including the observation of bilateral movements of the lip musculature, movements which have not been mentioned in the subsequent publications of Sherrington and his co-workers, need corroboration through further experiments.

The experiments of Beevor and Horsley on the orang constituted our only knowledge of the motor cortex of the great anthropoid apes until Grünbaum and Sherrington published two short preliminary accounts of their famous experiments on seven specimens, i.e. on the orang-utan (1901/02), the chimpanzee (1901/02, '04) and the gorilla (1901/02, '04).

A later publication by Roaf and Sherrington (1906) described experiments on another orang. An additional, short account of the stimulation experiment of an orang is included in C. and O. Vogt's work (1906/07).

It would be expected that the differentiation of the motor cortex in primates would parallel the general evolution of the brain. This was confirmed through the experiments. There is a decided advance in the anthropoid apes when compared with the gibbons and the catarrhine monkeys.

Grünbaum and Sherrington (1901/02, '04) found that the electrically responsive cortical motor area in the anthropoid apes unbrokenly occupies (this in contrast to Beevor and Horsley) the whole length of the precentral convolution, and in most places the greater part or the whole of its width (see also Sherrington, 1906, p. 272) (figs. 16–18). In contrast to the lower catarrhines it extends downward almost as far as the fissura lateralis Sylvii. At the upper mesial edge of the hemisphere the motor area extends over upon the mesial surface of the hemisphere and posteriorly into the depth of the sulcus centralis. This is the posterior limit of the responsive motor area. It was also demonstrated that the postcentral gyrus is electrically non-responsive (Grünbaum and Sherrington, C. and O. Vogt). This was confirmed through ablation experiments (Grünbaum and Sherrington, 1901/02). There were no obvious symptoms of paresis upon ablation of large parts of the postcentral gyrus. Moreover, subsequent histological investigations by Campbell (1905) on the orang and chimpanzee showed that the large pyramidal cells did not extend posterior to the sulcus centralis Rolandi. It was likewise demonstrated by Holmes and May (1909) that chromatolysis consequent to cutting of the corticospinal tract in the upper cervical region of the chimpanzee did not extend behind the sulcus centralis. Thus through stimulation and ablation experiments and through correlated histological studies it was definitely proven that the motor area in the anthropoid brain, as in the lower catarrhines and the platyrrhines, is restricted to areas anterior to the sulcus centralis Rolandi.

In addition to the surface of the motor area that is actually exposed, there is a very considerable excitable area hidden in the deep, individually variable sulci which are so characteristic of the anthropoid brain. Grünbaum and Sherrington judged this hidden area to be as large as the free surface of the motor area (Sherrington, 1906, p. 278). Later, however, Leyton and Sherrington (1917) expressed the opinion that the hidden area probably amounts to about one-third of the entire motor cortex. Sherrington (1906) indorsed Beevor and Horsley's view that the motor cortex of the anthropoid brain forms a relatively smaller fraction of the total surface of the

neopallium than it does in the lower catarrhines. But he emphasized the fact that the motor area has grown in absolute extent, and that its relative decrease is caused by the still greater increase of other areas belonging to the so-called "silent" fields. This vast expansion of inexcitable, richly convoluted fields is indeed one of the most outstanding features of the highly organized neopallium of the anthropoid brain. In this, the great anthropoid apes closely resemble man.

Within the excitable motor area of the anthropoid ape brain the regions representing the various parts of the body were found to be in an arrangement which largely corresponds to conditions in lower catarrhines and higher platyrrhines (Beevor and Horsley, Grünbaum and Sherrington, C. and O. Vogt, and subsequent authors). All investigators agree, however, that there is a greater integration of localized movements in the anthropoid ape as compared with the monkeys (Sherrington, 1906). More elaborate and very numerous movements from small subdivisions of the motor area can be obtained from the anthropoid brain. Thus, for example, from the arm area isolated movements of the individual fingers were obtained upon stimulation of certain points. Other small parts of the body were represented in a similar way. Hines (1929), in a critical review, states that "combination of discrete movements into a coördinated whole is more evident in the anthropoid cortex than in that of monkeys or of such laboratory animals as the rabbit, dog or cat."

In regard to the cortical representation of the mimetic musculature of man, comparative anatomical investigations on the catarrhine monkeys and the anthropoid apes are of great significance. In catarrhine monkeys, and also in anthropoid apes, the mimetic musculature not only of the upper but also of the lower part of the face, especially of the lips, has been reported to be in part bilaterally represented in the motor cortex (compare e.g. Beevor and Horsley, 1894, on the macaque; 1891, on the orang). Such bilateral movements of the lip musculature in the orang have not been confirmed by subsequent investigators. Contrary to Beevor and Horsley, Sherrington and his co-workers found that bilateral representation of the mimetic musculature in the anthropoid apes is restricted to and even accentuated in the muscles of the upper region of the face (i.e. the m. orbicularis oculi, mm. depressor and corrugator supercilii and m. frontalis), but most noticeable in the m. orbicularis oculi (Grünbaum and Sherrington, 1904; Leyton and Sherrington, 1917). In this respect the big anthropoid apes seem closely to resemble man, in whom, as seen from clinical cases and physiological observations, the mimetic muscles of the upper part of the face are evidently bilaterally represented. It is a well known fact that in man, in the facial palsy resulting from central lesions restricted to one hemisphere, the mimetic muscles of the upper part of the face are but little impaired, whereas there is a complete facial palsy in the lower part of the face. Thus, comparative investigations of the anthropoid apes and man bring clear evidence against the view of some authorities (see Krause, 1911, Bd. I, p. 157) that the bilateral innervation of these muscles is a subcortical one.

In exploring the motor cortex, Sherrington and his co-workers found it necessary to prick or tear small holes in the arachnoid to let out the cerebrospinal fluid, in order to avoid spread of current into areas adjacent to those under stimulation. Because of the characteristic rich pattern of convolutions, which are separated by deep sulci, and because of the closeness of the numerous small subdivisions of the motor field, stimulation experiments on the an-

thropoid brain require more precaution than do experiments on monkey brains. It is obvious that in the exploration of such a highly organized motor cortex only the unipolar method of stimulation could give satisfactory results.

Most interesting are the results of ablation experiments undertaken by Sherrington and his co-workers. Even the first communication by Grünbaum and Sherrington (1901/02) includes a short account of such experiments. Additional data, including references to the facial field, are given in their second communication (1904). Further statements are contained in Sherrington's book (1906), in the short publication of Graham Brown and Sherrington (1913a), and in Leyton and Sherrington's full presentation of the subject (1917). According to these accounts, extirpation of the arm area caused an immediate, severe crossed brachioplegia, which affected the fingers most. The paresis diminished rapidly, and in six weeks' time the animal had in large measure recovered the usefulness of the limb. Subsequent extirpation of the arm area of the second hemisphere produced no noticeable change in the paretic and partly recovered arm, but, as would be expected, resulted in a paresis of the opposite arm. A lesion of the leg area similarly caused temporary paresis of the opposite leg, especially in the toes and at the ankle joint. This lesion was smaller and the recovery more rapid than in the instance of the arm area lesion. Ablation of the facial area resulted in crossed hemiparesis in the lips, cheek, nasal fold, lower eyelid (very slightly), but not in the upper lid, eyebrow and frontal region. Recovery from these lesions occurred. Records of these experiments have been presented as moving pictures (Graham Brown and Sherrington, 1913b).

Sherrington and his co-workers offered no explanation as to what mechanism is involved in the restoration of the motor function after excision of the electrically excitable motor area. The fact that after ablation of the corresponding motor area of the other hemisphere no change in the partly recovered limb took place, definitely proves that the second hemisphere did not play a rôle in the recovery of the originally affected limb (i.e. the limb contralateral to the cerebral lesion). On the other hand, the regained voluntary motility of the extremities, especially of the upper limb, and notably of the hand and fingers, is of such a complex nature that it somehow suggests cortical control as normally exists before the operation. It seems possible that the ablated portions representing the electrically excitable motor areas do not really cover the potential motor field, which may be much more extensive than it would appear from stimulation experiments. Thus Rothmann (1905) claimed that the actual motor area is far greater than that which is electrically excitable; in particular, he judged the upper extremity area to be of enormous extent. Sherrington did not seem to consider it likely that the remaining, perhaps less specialized, adjacent potential motor areas of the cortex take over the function after excision of the electrically excitable zones. In the experiments of Sherrington and his co-workers (e. g. in the case of ablation of the hand area) neither the ablation nor excitation methods gave evidence that the remaining part of the arm area had taken over the function of the extirpated hand region. Nor was the postcentral gyrus especially altered under experimentation; it had not become an area stimulable for arm, hand, or other movements, nor did subsequent ablation of the adjoining postcentral region cause recrudescence of the arm paresis. The authors likewise demonstrated that recovery of function was not the result of regeneration of the areas de-

stroyed (compare Leyton and Sherrington, 1917, pp. 207 ff.). Thus this fundamental question remains unsettled and suggests renewed investigation.

In connection with ablation experiments, Fulton and Keller (1932) mapped out the electrically responsive cortex in chimpanzees. After ablation of the leg area, the Babinski reflex appeared in two phases. Paresis was more profound and more lasting than in the monkeys or the gibbon. The indications were that "cortical dominance" in the chimpanzee is more complete than in the other primates studied, and in this respect the chimpanzee more closely approaches man. Removing the second (opposite) leg area in some of their primates, Fulton and Keller found that such an operation produced but little influence on the originally affected extremity in all of the monkeys excepting the baboon. This animal suffered definite and permanent impairment of motor power, as did the gibbon and the chimpanzee. This led them to conclude that baboon, gibbon and chimpanzee possess some degree of bilateral representation in the leg area, indicating that some of the corticospinal fibers to the lower extremity never cross.

While in the anthropoid apes a gradual and almost complete recovery from extirpation of the motor area takes place, in man the effects of destruction of the motor cortex are of far graver consequence. The results of ablation experiments in monkeys and anthropoid apes, correlated with the data from numerous cases of paralysis in human patients, show that in the ascending scale of the primates the motor cortex gradually takes over complete control of the voluntary movements and not merely their regulation. Through the fundamental investigations of Graham Brown and Sherrington (1912) it becomes clear that the reactions from the motor cortex are very complex and composite as compared with decerebrate or purely spinal reactions (see further Hines, 1929, pp. 479–482). The data obtained are invaluable for a clearer understanding of the highly complex voluntary actions in man. A discussion of these problems is given by Leyton and Sherrington (1917, pp. 176–180; see also v. Monakow, 1914).

Sherrington and his co-workers have given special attention to the "echo-responses" from the postcentral gyrus. Grünbaum and Sherrington (1901/02) mentioned that in the anthropoid apes feeble reactions may under certain circumstances be obtained from the postcentral gyrus, especially through strong faradization. However, they emphasized the fact that these responses were equivocal and occurred under circumstances that exclude their acceptance as equivalent to motor reactions. Graham Brown and Sherrington later (1912) formulated this more clearly, stating that points in the postcentral gyrus, when stimulated immediately after cessation of a stimulus of a point of the precentral gyrus, are frequently able to elicit some return of the movement obtained just previously from the precentral convolution, producing, so to speak, an echo of the preceding response. But this dies out usually after the first repetition (see also Graham Brown, 1914; Leyton and Sherrington, 1917). Ablation of a large portion of the postcentral gyrus gave no paralysis, not even transitory (Grünbaum and Sherrington, 1901/02). Thus a chimpanzee, in which the postcentral convolution of the right cortex had been freely destroyed opposite the arm area, gave the hand at command ninety minutes after he came out of ether (Graham Brown and Sherrington, 1913).

Graham Brown, applying the method of rapid freezing of small points of the cortex with the aid of liquid air, then demonstrated (1914) that the activity of

the postcentral gyrus must be physiological and not due to spread of current to the corresponding section of the precentral gyrus (compare C. and O. Vogt), except in cases where too strong a current is used. He discussed the possible explanations of this peculiar phenomenon (Graham Brown, 1914, p. xxx). Thus it seems quite possible that the postcentral gyrus has a modifying influence on the corresponding directly excitable section of the precentral gyrus.

C. and O. Vogt (1906/07, p. 361) emphasized the fact that destruction of the postcentral gyrus in the monkey led to a decrease of the adequate characters of movements. The characteristic behavior of a monkey with the postcentral gyrus destroyed, in contrast with one in which the precentral gyrus was ablated, had previously been vividly portrayed by Brodmann (1905). Also, Lewandowsky and Simons (1909) noticed marked ataxia after removal of the postcentral gyrus, which they, however, attributed to sensory disturbances (compare on the other hand Leyton and Sherrington, 1917, pp. 209–210, 221).

That the electrically non-excitable precentral region has a direct modifying influence upon motor actions, becomes evident from the Vogts' additional observation that destruction of non-excitable precentral areas resulted in loss of certain complex limb movements (1906/07, p. 361). In human beings it has moreover been observed that disease of the frontal lobes causes a very striking reversion from the complex motor behavior to a more primitive behavior pattern, because lower mechanisms are released after damage to the dominating higher centers (Freeman, 1929).

The publication by Leyton and Sherrington (1917), giving a full account of the previous and additional, most extensive and most careful investigations of Sherrington and his co-workers, crowned the investigations on the motor cortex of the great anthropoid apes. This work includes the results and conclusions from experiments on 3 orang-utans, 22 chimpanzees and 3 gorillas. Thus the motor cortex of the great anthropoid apes has become the best known of the whole primate stock. The three great anthropoid apes seemed closely to resemble each other in regard to the motor cortex. But there were great individual variations and differences in the smaller details of localization of the right and left motor cortex of the same animal. The simpler examples of chimpanzee brains seemed inferior in development to well-developed examples of the brain of the orang, while in general the orang was found to be a little below the chimpanzee. The largest and most highly developed brain examined by Sherrington and his co-workers was that of a gorilla. The motor area in that specimen appeared to be, on the whole, the most extensive and most differentiated of those experimented upon (Leyton and Sherrington, 1917, p. 219).

In respect to the general development of the fore brain, some investigators place the orang-utan above the chimpanzee and the gorilla (compare e. g. Jacob and Onelli, 1911; Riese, '26; etc.). In more recent times the mentality of the chimpanzee has been studied by psychologists (Köhler, Yerkes, Kohts and others). The gorilla and the orang, on the other hand, less accessible and less coöperative in experimental work than the chimpanzee, are as yet little known and little understood. Some authorities, however, consider the orang to be of higher intelligence than either the chimpanzee or the gorilla.

In the orang-utan the facial area in relation to the rest of the motor area appears to be more extensive than in the other anthropoids (see figs. 16–18). It was found

to be considerably longer from above downward than in the chimpanzee and gorilla (Sherrington, 1906; Leyton and Sherrington, 1917). Apart from this distinction, there seem to be no clear differences between the motor areas of the three types of the big anthropoid apes (Leyton and Sherrington, 1917). That the motor cortex of the orang-utan would, at least to some extent, differ from that of the chimpanzee and of the gorilla, might be expected in view of the fact that in other organ systems the orang differs widely from his anthropoid cousins, the chimpanzee and gorilla, who closely resemble each other. This has been found most striking in a close comparison of the mimetic musculature of the three types (Ruge, 1887; Huber, 1931a). Renewed comparative studies on the motor cortex of the three big anthropoid apes may possibly bring out further differences.

d) Man

The first attempts to investigate the motor cortex of the human brain were made by Bartholow in 1874. He experimented with both faradic and galvanic currents, using insulated needle electrodes, which were applied to the dura mater and also inserted directly into the brain substance. Movements were obtained from the head, neck and extemities, but these responses were not referred to localized areas in the cortex.

In succeeding years a series of reports of more exact observations were published by various authors, chiefly by American neurologists and surgeons, among which those of Keen, Lloyd and Deaver, and Mills are notable for the exact manner in which faradization was used, as early as 1888, as an aid in localizing the motor area of the cortex in connection with brain surgery. In 1897, Lamacq published an important paper on the motor cortex of the human brain, including a discussion of the observations of previous workers. More recently, most valuable contributions to our knowledge of the excitable human cortex have been made by Krause (1904, '05), Mills and Frazier ('05), Cushing ('09), van Valkenburg ('14), and Foerster ('26, '31) (see figs. 19 and 20).

Encouraged by the experiments of Grünbaum and Sherrington upon anthropoid apes, Krause (1904, '05) stimulated a number of human brains by unipolar faradization. He stressed the fact that, owing to the great variability of the pattern of the convolutions, pure anatomical orientation with a comparatively limited exposure of the cortex (arachnoid and pia mater being intact) is impossible. He warmly advocated the physiological method of electrical stimulation as the only means of exact localization in the operative treatment of Jacksonian epilepsy.

Mills and Frazier (1905) independently followed the same line of work, for the most part using unipolar faradization upon anesthetized patients.

Later Cushing (1909) and van Valkenburg (1914) presented most valuable data obtained from patients whom they had stimulated by the unipolar method in the conscious state. Their observations give evidence that in man the electrically excitable motor and the sensory areas are separated by the sulcus centralis. This, however, does not necessarily lead to the conclusion that the motor and sensory areas are so sharply separated in a physiological sense. There is evidence that even in man the sensory area extends in front of the sulcus centralis into the motor area. This has been convincingly demonstrated in the macaque (*Macacus rhesus* and *M. nemestrinus*) by Dusser de Barenne (1924). Van Valkenburg, moreover, found that the sensible points for given parts of the body in the postcentral gyrus lie exactly parallel

to the corresponding motor points in the precentral gyrus (compare also Foerster, 1931; and fig. 20). This characteristic arrangement is probably not solely due to a physiological coördinating influence of cortical sensibility upon motion (v. Valkenburg), but is presumably determined through morphological segregation of the motor and the sensory areas in the higher mammals by gradual evolution from a less differentiated sensory-motor area typical of the primitive mammalian ground-plan. Extensive comparative investigations on cortical localization by Brodmann strongly suggest such evolution.

Foerster (1926) studied the motor cortex in connection with operations on epileptics. A more recent report of his work (1931) is of great importance (see fig. 20). He stimulated patients under local anaesthesia with threshold galvanic stimuli and under general anaesthesia with faradic current. In order to obtain circumscribed effects he found it essential that the cortex be dry and that the stimuli be of threshold strength. The threshold of the foci varied from one individual to another. He classified the motor areas of the cortex as pyramidal (corticospinal) and extra-pyramidal. The pyramidal area is confined to area 4, whose cells give rise to the fibers of the corticospinal tract. This area lies chiefly in the central sulcus and also in the precentral gyrus. It is the specific region for isolated innervations. Stimulation of points in this area produces isolated movements of a single segment of the limbs or of a single portion of the trunk or head, usually contralateral. Representation of the upper portion of the face, pharynx, vocal cords and masticatory musculature is bilateral. Stimulation of the trunk and thigh areas also occasionally produces bilateral movements of these parts. Movements of the sternocleidomastoid and external pterygoid muscles are ipsilateral. There are separate foci for each finger. Separate innervation may be distinguished not only for single groups of muscles but likewise for individual muscles as well. When area 4 or the corticospinal tract is destroyed, isolated movements are no longer performed, yet voluntary movement is by no means completely abolished. In such a lesion the individual is still able to perform complex flexor or extensor movements of the affected limbs, but the movements are combined (e.g. in spastic tetraplegia voluntary movement of a single limb also produces movements of the other three extremities). Such movements arise in the extrapyramidal areas (6aB, 5a, 5b, 22, 6aA, 3, 1, 2). These regions do not give rise to fibers of the corticospinal tract, but are connected to subcortical and other lower centers; they are the areas for "adversive" and complex synergic movements which are in marked contrast to the isolated effects obtained from area 4. As long as the pyramidal system (area 4 and the corticospinal tract) is intact, stimulation of extrapyramidal areas 6aA and 3, 1, 2 produces isolated effects similar to those obtained from area 4, but Foerster showed that these movements are due to spread to area 4 (this is made clear by cases of Jacksonian epilepsy or by epilepsy resulting from lesions of the precentral gyrus consequent to destruction of the corticospinal tract, and by isolating area 4 by cortical incision or superficial abrasion of area 6aA). It is interesting to note that area 6aA in man occupies the greater portion of the summit of the precentral gyrus, whereas in monkeys, according to Foerster, it lies in the frontal convolution. It is likewise noteworthy that areas 3, 1 and 2, which Foerster ascribes to the extrapyramidal system, are located in the somaesthetic area, which lies in the postcentral gyrus. Foerster concluded that both pyramidal and extrapyramidal areas

coöperate in the performance of voluntary movements, but that isolated effects arise only from the pyramidal area (area 4). This area therefore is to be regarded as the "motor cortex" as defined in this paper. In respect to its location and subdivision Foerster's investigations confirm and extend the results of previous workers on man.

For obvious reasons, these experimental data obtained from man cannot be as complete as those produced from experiments upon other animals. It is important that the results so far obtained closely correspond to the findings from the more numerous and more complete experiments on the anthropoid apes. The true motor (pyramidal) area of the human cerebral cortex lies in the region anterior to the sulcus centralis Rolandi, a large portion, as in the apes, forming the walls of this sulcus. The localization clearly follows the plan in other primates, the hind limb area bordering the superior longitudinal fissure, with the head area adjoining the fissure of Sylvius (see figs. 19 and 20). The results obtained from stimulation are in accord with studies on the cyto-architecture of the human and anthropoid cortex (Campbell, 1905; Brodmann, '09; v. Economo and Koskinas, '25) and with localization of the cells of origin of the corticospinal tract (Holmes and May, 1909).

Hines (1929), after considering the data obtained from stimulation, ablation and accidental lesions, concluded that

> in spite of the great similarities of the motor cortex in ape and in man the importance of its intactness for the maintenance of normal posture in the latter is apparently greater . . . Seemingly the motor cortex presides over fine individual movements, especially of the extremities. It regulates the distribution of tone in opposing muscle groups. It exerts apparently a certain restraint on reflexes and associated movements. It integrates, or perhaps interprets, certain incoming impulses which are necessary for the coördination of movements and movement sequence. Moreover, it facilitates, augments and aids in gauging the amount of contraction of individual muscles or groups . . . The motor cortex may not be the site of origin of voluntary movement nor the gate through which learned movements pass, but certainly it assures to man that his muscular adjustments take place in the way to which he is accustomed and without undue effort or strain.

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FIGS. 1–7. LEFT CEREBRAL HEMISPHERES OF VARIOUS MAMMALS, SHOWING THE ELECTRICALLY RESPONSIVE AREAS OF THE CORTEX

The figures, having been taken from different sources, are not drawn to the same scale. They therefore are not indicative of relative size in comparison to one another.

FIG. 1. Duckbill platypus (*Ornithorhynchus anatinus*). (According to experiments by Martin, 1898/99.) Side view. In this animal facial and fore limb areas partly coincide, while hind limb and tail areas are absent.

FIG. 2. Opossum (*Didelphys virginiana*). (Modified after Weed and Langworthy, 1925, and other authors.) Side view. The undisputed motor areas border on the orbital sulcus.

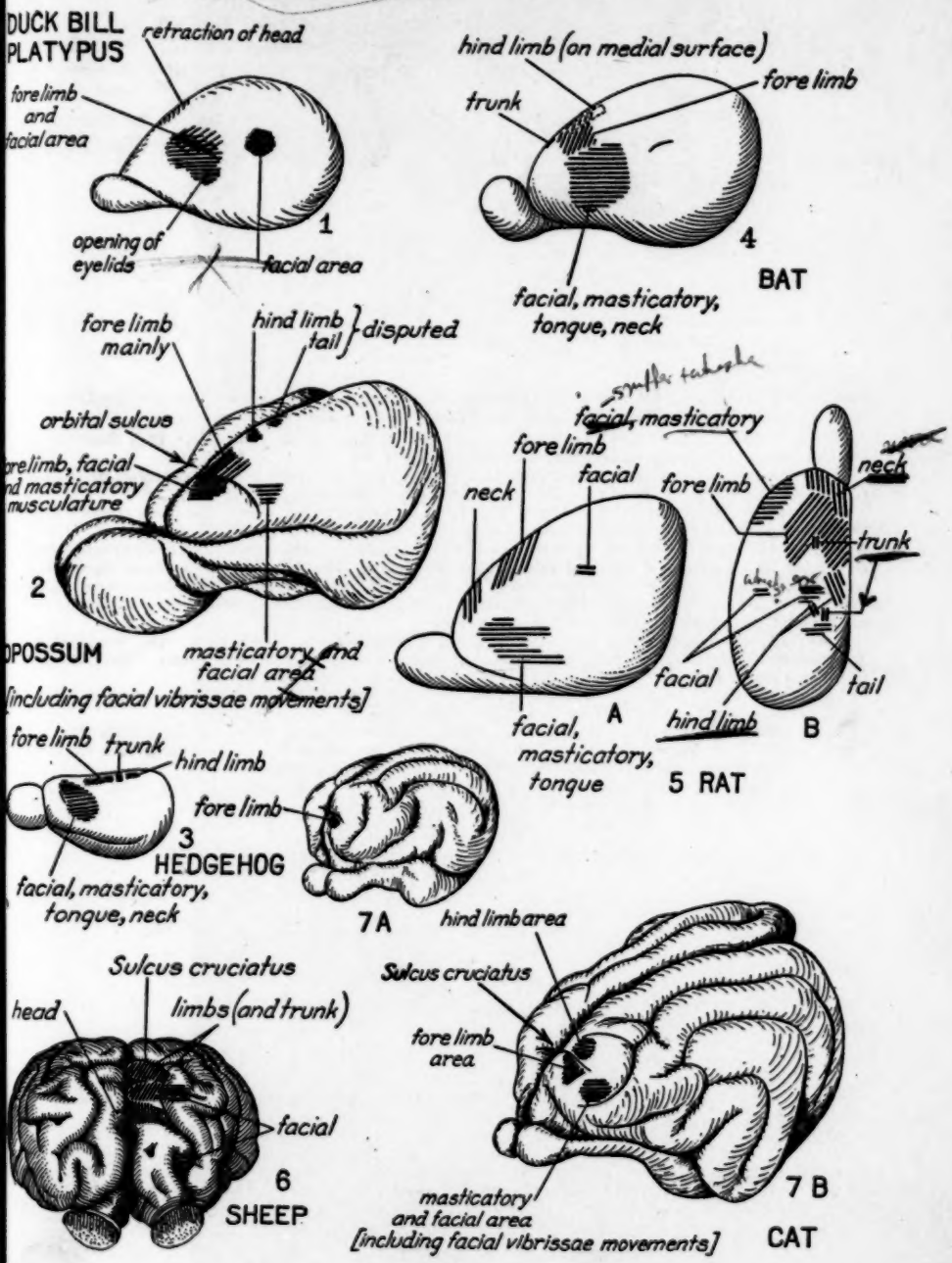
FIG. 3. Hedgehog (*Erinaceus europaeus*). (After C. and O. Vogt, 1906/07.) View from the side and slightly from above.

FIG. 4. Bat (*Pteropus Edwardsi*). (After C. and O. Vogt, 1906/07.) Side view. The inconstant hind limb area is located on the inner surface of the hemisphere.

FIG. 5. Rat (*Mus rattus*). (After Leahley, 1921.) A.—Side view. B.—View from above.

FIG. 6. Sheep (*Ovis*). (After Bagley, 1922.) Brain seen from in front, with electrically responsive areas charted on left hemisphere only.

FIG. 7. Cat (*Felis domestica*). A.—Newborn kitten (after Weed and Langworthy, 1926). Side view. At this age the only responsive area is that of the fore limb, which has the same location as in the adult cat. B.—Adult cat (after Weed and Langworthy, 1926, and after unpublished observations by Huber and Smith). Side view. The electrically responsive areas are grouped about the cruciate sulcus, an arrangement characteristic of carnivores.


DUCK BILL PLATYPUS
retraction of head
fore limb and facial area
opening of eyelids
facial area
1
hind limb (on medial surface)
fore limb
trunk
4
BAT
facial, masticatory, tongue, neck
fore limb mainly
hind limb
tail
disputed
orbital sulcus
fore limb, facial and masticatory musculature
2
POSSUM
masticatory and facial area
[including facial vibrissae movements]
facial, masticatory
fore limb
facial
neck
fore limb
neck
trunk
facial
tail
facial, masticatory, tongue
A
hind limb
B
5 RAT
fore limb
trunk
hind limb
fore limb
3
HEDGEHOG
facial, masticatory, tongue, neck
7A
hind limb area
Sulcus cruciatus
head
limbs (and trunk)
facial
6
SHEEP
Sulcus cruciatus
fore limb area
masticatory and facial area
[including facial vibrissae movements]
7 B
CAT

FIGS. 1–7

FIGS. 8–15. LEFT CEREBRAL HEMISPHERES OF PROSIMIANS, PLATYRRHINE AND CATARRHINE MONKEYS, AND GIBBONS, SHOWING THE ELECTRICALLY RESPONSIVE AREAS OF THE CORTEX

The figures, having been taken from different sources, are not drawn to the same scale. They therefore are not indicative of relative size in comparison to one another.

FIG. 8. Lemur (*Lemur macaco*). (After C. and O. Vogt, 1906/07.) Side view. The motor cortex lies in front of a small sulcus which possibly foreshadows the central sulcus of monkeys, apes and man.

FIG. 9. Potto (*Perodicticus potto*). (After C. and O. Vogt, 1906/07.) Side view. The motor cortex is situated in frong of sulcus "ce," possibly homologous to the central sulcus of simians.

FIG. 10. Marmoset (*Callithrix*). (After C. and O. Vogt, 1906/07.) Side view. The lissencephalic hemisphere has no suggestion of a central sulcus. The motor area, however, exhibits the same topographic relations as in those monkeys which possess a well defined central sulcus. The cortex is differentiated histologically (Brodmann, 1909). In the phylogeny of the primate cortex the development of the cyto-architecture precedes the formation of the sulci and gyri.

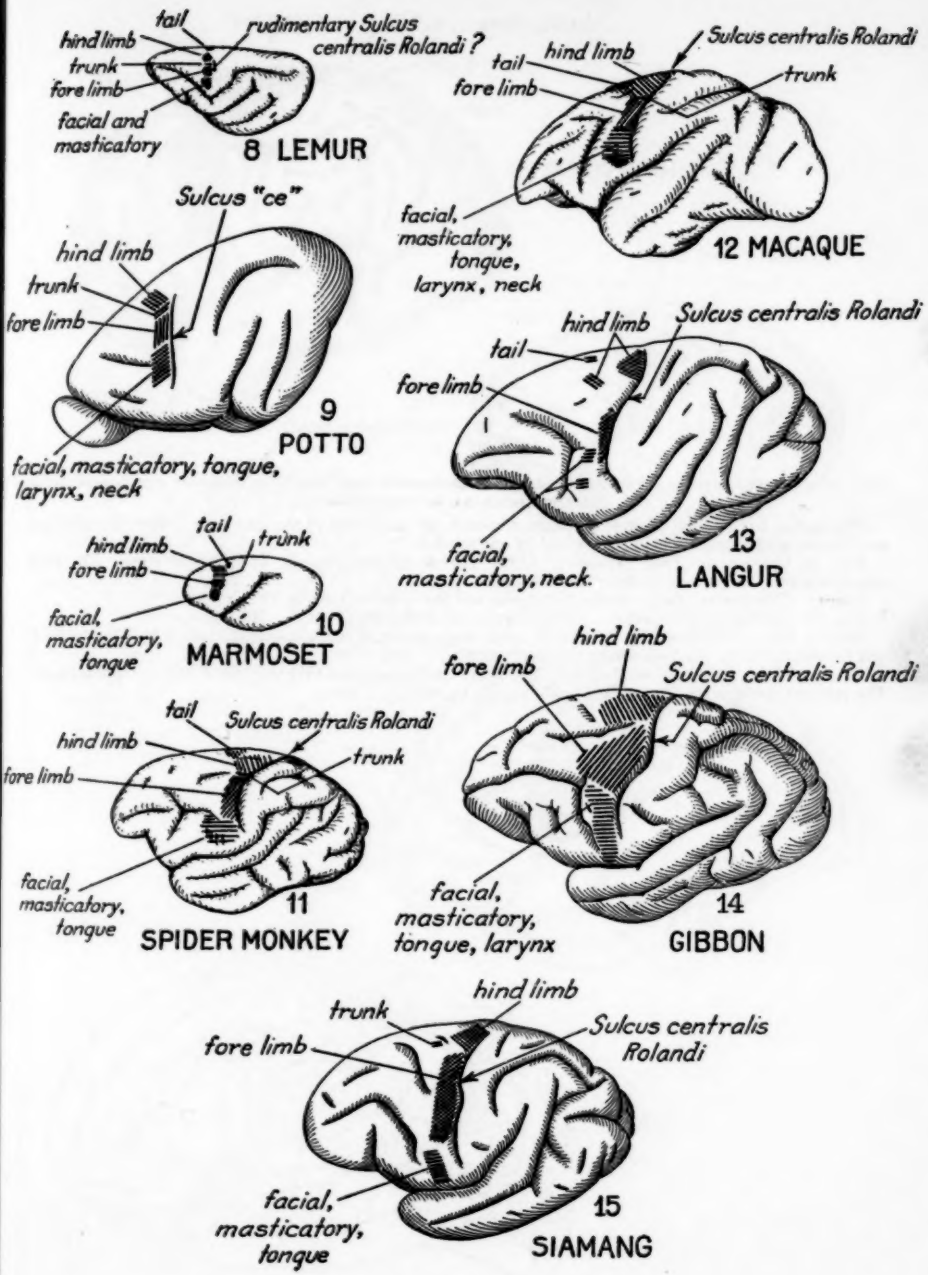
FIG. 11. Spider monkey (*Ateles ater*). (After C. and O. Vogt, 1906/07.) Side view. The central sulcus, as in the catarrhines, is well-defined, and it marks the posterior border of the motor cortex. The prehensile tail is well represented.

FIG. 12. Macaque (*Pithecus*). (Combined drawing from stimulation experiments by various authors, including unpublished observations by Huber and Smith.) Side view.

FIG. 13. Langur (*Semnopithecus* (= *Pygathrix*) *entellus*). (After C. and O. Vogt, 1906/07.) Side view.

FIG. 14. Gibbon (*Hylobates*). (After Mott, Schuster and Sherrington, 1911.) Side view. The fore limb area is well-developed, and is expanded forward into the middle frontal convolution.

FIG. 15. Siamang (*Symphalangus syndactylus*). (After C. and O. Vogt, 1906/07.) Side view. The fore limb area is relatively huge in its extent.


tail
hind limb
trunk
fore limb
facial and masticatory
rudimentary Sulcus centralis Rolandi ?
8 LEMUR
Sulcus centralis Rolandi
hind limb
tail
fore limb
trunk
facial, masticatory, tongue, larynx, neck
12 MACAQUE
Sulcus "ce"
hind limb
trunk
fore limb
9
POTTO
facial, masticatory, tongue, larynx, neck
hind limb
Sulcus centralis Rolandi
tail
fore limb
facial, masticatory, neck.
13
LANGUR
tail
trunk
hind limb
fore limb
facial, masticatory, tongue
10
MARMOSET
hind limb
fore limb
Sulcus centralis Rolandi
tail
Sulcus centralis Rolandi
hind limb
trunk
fore limb
facial, masticatory, tongue
11
SPIDER MONKEY
hind limb
fore limb
Sulcus centralis Rolandi
facial, masticatory, tongue, larynx
14
GIBBON
hind limb
trunk
Sulcus centralis Rolandi
fore limb
facial, masticatory, tongue
15
SIAMANG

FIGS. 8–15

FIGS. 16–20. LEFT CEREBRAL HEMISPHERES OF GREAT ANTHROPOID APES AND MAN, SHOWING THE ELECTRICALLY RESPONSIVE AREAS OF THE CORTEX

The figures, having been taken from different sources, are not drawn to the same scale. They therefore are not indicative of relative size in comparison to one another.

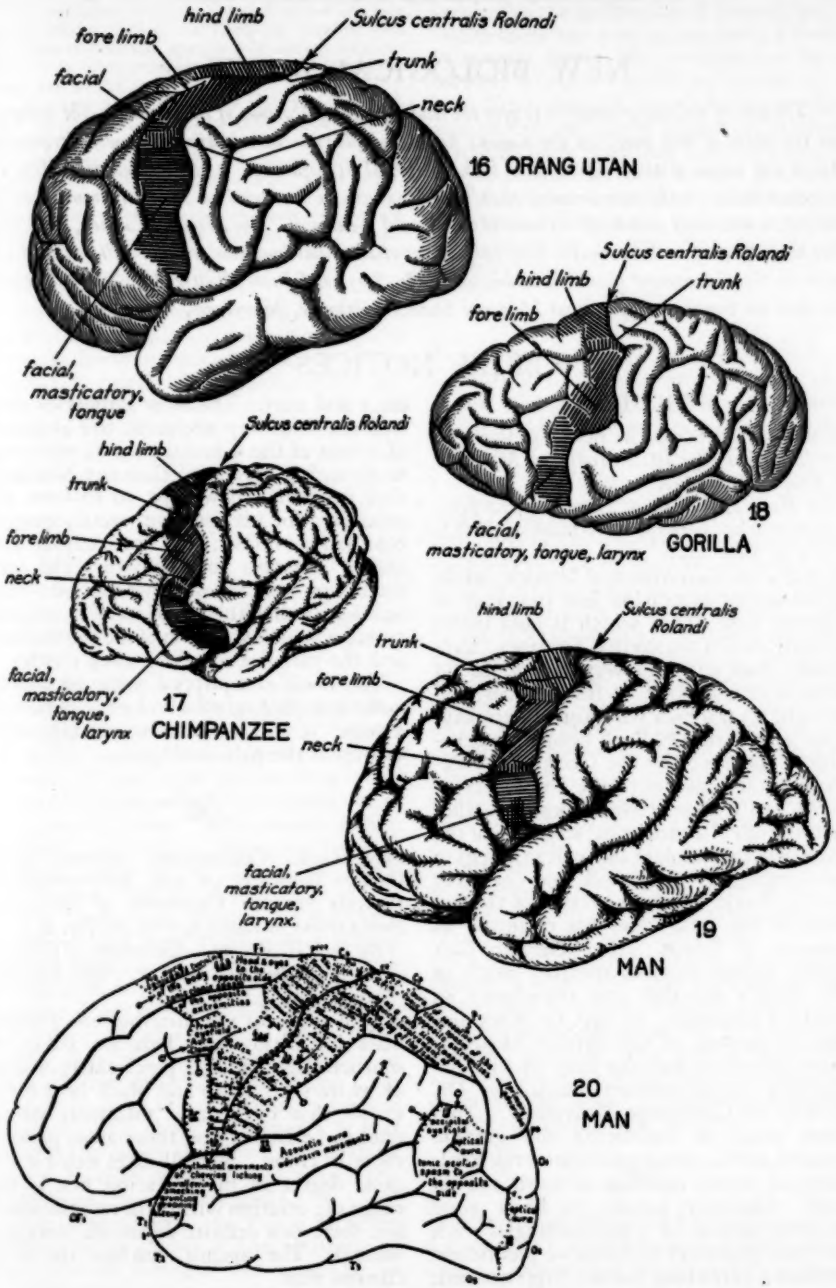
FIG. 16. Orang-utan (*Pongo pygmaeus*). (After Leyton and Sherrington, 1917.) Side view. The head area is relatively larger than in the other anthropoid apes.

FIG. 17. Chimpanzee (*Pan*). (After Grünbaum and Sherrington, 1901/02.) Side view.

FIG. 18. Gorilla (*Gorilla gorilla*). (After Leyton and Sherrington, 1917.) Side view.

FIG. 19. Man. (Outline from Spalteholz' atlas, with results of stimulation experiments by Krause, Mills and Frazier, Cushing, van Valkenburg, etc. superimposed.) Side view.

FIG. 20. Man. (After Foerster, 1931.) Side view. Area 4 represents the true motor field (pyramidal area). The extrapyramidal areas are numbered 6aB, 5a, 5b, 22, 6aA, 3, 1 and 2.


hind limb
Sulcus centralis Rolandi
fore limb
trunk
facial
neck
16 ORANG UTAN
facial, masticatory, tongue
Sulcus centralis Rolandi
hind limb
trunk
fore limb
18
facial, masticatory, tongue, larynx
GORILLA
Sulcus centralis Rolandi
hind limb
trunk
fore limb
neck
facial, masticatory, tongue, larynx
17
CHIMPANZEE
hind limb
Sulcus centralis Rolandi
trunk
fore limb
neck
facial, masticatory, tongue, larynx.
19
MAN
20
MAN

FIGS. 16–20

NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of biology. In addition there will frequently appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to Dr. Raymond Pearl, Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *1901 East Madison Street, Baltimore, Maryland, U. S. A.*

BRIEF NOTICES

EVOLUTION

OLDOWAY, DIE SCHLUCHT DES URMENSCHEN. *Die Entdeckung des altsteinzeitlichen Menschen in Deutsch-Ostafrika.*

By Hans Reck. F. A. Brockhaus, Leipzig. 8.70 marks (paper); 10.50 marks (cloth). 6 x 9; 308; 1933.

In 1911 a Dr. Kattwinkel of Munich, while on a hunting expedition into that part of German East Africa which is now incorporated into Tanganyika Territory, happened upon the Oldoway gorge, found there a rich deposit of fossil bones, and brought several back with him to Germany. Two years later Dr. Reck headed an expedition, sponsored by the Geological-Paleontological Museum of the University of Berlin, to explore these deposits further and to study the geological structure of the gorge and the desolate and volcanic region surrounding it. The work was cut short by the World War, the service of the author in the army, and his retention as prisoner in Egypt. After German East Africa became English territory much of Dr. Reck's material was transferred to England for study. In 1931 Dr. Reck and Mr. Hopwood, of the British Museum, were invited to join the East African Archaeological Expedition, headed by Dr. Leakey of Cambridge University. This book gives an interesting and graphic account of the two expeditions—the hardships of travel, methods of work and results. Oldoway proved to be a good hunting ground for a paleontologist with its vast profusion of bones of prehistoric animals—three-toed horses, hippopotami, etc.—and man. The most important find was the Oldoway skeleton, the skeleton of a man of the diluvial period, who was more highly developed than the Neanderthal man and knew how to fashion instruments for his use from wood, quartz, obsidian and lava. In one chapter the author attempts to reconstruct the surroundings and life of this period. His opinion is that the eruption of a volcano caused a mass destruction of the inhabitants and the paradise in which they lived.

The book is equipped with an index, numerous photographs and drawings, and a map. It will interest the general reader as well as the paleontologist.

CONCEALING COLORATION AMONG SOME DESERT RODENTS OF THE SOUTHWESTERN UNITED STATES. *University of California Publications in Zoology, Vol. 40, No. 1.*

By Seth B. Benson. University of California Press, Berkeley. $1.25. 6¾ x 10¼; 69; 1933 (paper).

A field study of the distribution of mammals inhabiting the Tularosa Basin of southern New Mexico, particularly an area of white sand dunes and black lava flow, shows that only those mammals which tend to be isolated on these areas parallel them in color. Not all races exhibit the same degree of pigmentation nor is the range of variation within the race the same, but there is a definite departure from the normal. The author reaches the conclusion that

The hypothesis of the operation of natural selection toward producing protective coloration, together with the factor of isolation, appears to be the best explanation of the development of the races peculiar to the dunes and to the lava.

The extremely wide range of variation present in some of the races inhabiting these special areas is probably due to the fact that the barriers which enclose them are not impassable, even to animals which tend to to be restricted to dunes and [or] rocky places, thus allowing "normal" individuals from the surrounding area to intermingle and interbreed with the peculiar races. It is possible, however, that the wide range of variation may mean that selection has not yet established a constant race.

The study includes a general account of mammals present in the lower parts of the Tularosa Basin, meteorological data of the region and detailed descriptions of the important features of the lava beds and sand dunes, with illustrations. There is also a literature list of 49 titles and a number of figures and plates.

EVOLUTION AND REDEMPTION.

By H. P. Newsholme. Williams and Norgate, London. 8s. 6d. net. $4\frac{7}{8}$ x $7\frac{1}{2}$; 267; 1933.

This book sets forth a very pious version of Lamarckism. In the beginning God created spirits, to whom He gave "freedom to attain expression of themselves, and thereby of God from Whom they were derived, through a material created out of their own substance." By misuse of their freedom, either from cowardice or from self-will, these spirits introduced a bias into matter, which manifests itself as inertia, death, sin, disease and suffering. Evolution has been a process of casting off this inertia. Those organisms whose strivings were in harmony with the immanent divine element developed along the main track of evolution, while those which were motivated by cowardice or self-will diverged into blind alleys. But even in the main line of evolution the bias of inertia still clung to matter. It was therefore necessary that the Creative Spirit should become Man, should take upon Himself and overcome inertia.

In his discussion of the Virgin Birth the author concludes that "The irruption of the spiritual through the material, in breaking the inertia of the latter may break also the uniformity of natural law." But is there any need of assuming a break in natural law? We are informed by a medical friend, formerly an interne in a New York obstetric ward, that a considerable proportion of the unmarried mothers delivered there state that theirs is a case of virgin birth. If then human parthenogenesis is a not uncommon phenomenon in the twentieth century, need we interpret its occurrence in the first century as miraculous?

TROIS PROBLÈMES: L'ESPÈCE—L'INSTINCT—L'HOMME: *L'Allure du Développement, Critérium d'Espèce; La Synergie Fonctionelle Automatique, Source de l'Instinct; l'Homme, Anthropoïde Ralenti de Développement.*

By Émile Devaux. Librairie E. Le François, Paris. 25 fr. $6\frac{3}{8}$ x 10; xii + 350; 1933 (paper).

The ideas of Darwin, Lamarck and others are freely borrowed in this book which according to the author expresses a novel view of evolution. His theory is founded on the fact that each species has a different rate of development (*allure du développement*). A change in this rate causes mutation and is due to hunger and other physical hardships which affect the organism and the sexual organs in particular, and through these the germ cells. The slower the rate of development, prenatal especially, the higher in the scale of evolution rises the animal. Man is thus an animal with retarded development, but he is superior to the others with equal or slower rates because of the preponderance of the brain and nervous system.

The object of this book is to present this theory in the hope that experiments be undertaken to prove or disprove it.

The naïveté of the writer is refreshing indeed. After all, why build a theory on arrays of statistics, experiments or multiple observations when ingenious inferences satisfy the author just as well?

STUDIES OF THE PLIOCENE PALAEOBOTANY OF CALIFORNIA. *Contributions to Palaeontology, Carnegie Institution of Washington Publication No. 412.*

Carnegie Institution of Washington, D. C. 6⅞ x 10; 134 + 18 plates; 1933.

The first and longer paper in this volume is on *Pliocene Floras of California*. Erling Dorf reports fossil plant remains of 34 species taken from 16 localities. Ten of these localities are less than 35 miles from San Francisco Bay, the other six at much farther points to the north and south. The formations in which they occur are of lower to middle Pliocene age. The occurrence of the plant bearing horizons within each of the various formations is treated in detail. The study is documented and indexed.

The second paper is on *Woods from the Ricardo Pliocene of Last Chance Gulch, California* by Irma E. Webber. This is based on a field and laboratory study of petrified wood. The species represented are all similar to or possibly identical with those now living in the southern United States. Both papers are excellently illustrated.

EVOLUTION OF HABIT IN BIRDS.

By Edmund Selous. Constable and Co., London. 10 shillings net. 5 x 7¾; xvi + 296; 1933.

The author writes of such matters as formalization and the disappearance of the fighting instinct, nest-building, sexual display, landed proprietorship, domestic cleanliness, parental care, song, and the storing habit of the California woodpecker. But his ideas and discussions of their origin and development are essentially similar to those of the Romanes-Haeckel-Weismann era. Many detailed observations taken from field notes on a variety of species of birds are offered to illustrate the conclusions. The book is written in the author's usual charming, though at times difficult, style. The index is adequate, well-prepared, and, one might say, as interestingly written as the rest of the book.

THE PRINCIPLES OF HISTORICAL GEOLOGY FROM THE REGIONAL POINT OF VIEW.

By Richard M. Field. Princeton University Press, Princeton, N. J. $3.50. 6 x 9; xii + 283 + 10 plates; 1933.

The plan on which this textbook is laid out is to introduce students to the working methods of historical geology by discussing in turn six regions, the Grand Canyon, Niagara Falls, the Appalachians, the Northwest Highlands of Scotland, the Alps, and the Yellowstone Park, beginning with localities in which the succession of geological events is easily traced and proceeding to more obscure situations. The biological aspect is largely omitted. The illustrations are block diagrams and topographical maps and they include drawings of relief maps of the regions described. Interest has been added by notes on the adventures of the pioneer geologists who worked in these regions and by an entertaining chapter on the history of stratigraphy. It can be recommended to anyone interested in "the scientific interpretation of scenery" and to scientifically-minded tourists.

MERKMALE UND GRENZEN IN DER DOMESTIKATIONSFRAGE AM GEBISS. *Deutsche Zahnheilkunde Heft 84.*

By Heinrich Fabian. Georg Thieme, Leipzig. 8 marks. 6⅛ x 9¾; 99; 1933 (paper).

Although most of the defects of teeth and jaws of wild and domesticated animals considered in this summary of the comparisons made by the author and others, were found to be more prevalent in the domesticated forms, animals living in the wild may, and do, acquire such defects under certain conditions, *e.g.* scarcity of necessary food elements. *Ergo*, the author concludes that it is not domestication itself or the length of time involved that is essential. It is rather the manner in which it has been carried out, from environmental and dietary points of view. The book is provided with a bibliography of 3 pages and many excellent photographs.

PHYLOGENESE UND GESCHWULSTENTSTEHUNG.

By Max Glogner. Johann Ambrosius Barth, Leipzig. 1.60 marks. 6 x 9⅛; 32; 1933 (paper).

A presentation of the author's theoretical

views on the etiology of malignant tumors. On the basis of experimental studies on the heredity of malignant disease, the changes in its development, and the similar behavior, morphologically and physiologically, of protozoan and malignant cells, the author favors the phylogenetic theory according to which the malignant cells are thought to be descended from the protozoan cells from which the early multicellular organisms are held to have been formed. Although most of these cells are transformed into metazoan cells some retain protozoan characteristics, and thus the protoplasm of malignant cells has been transmitted continually, from the earliest times of organic life, by the germinal substance of the sex cells. The brochure lacks index and bibliography, and, we venture to think, probatory cogency.

TELEOLOGICAL FACTORS IN EVOLUTION. *Function Determines Structure. A Thesis Presented to the Faculty of the American University Graduate School in Partial Fulfillment of the Requirement for the Degree of Doctor of Philosophy, Washington, D. C., 1932.*

By Leon A. Fox. [*Limited number of copies for free distribution; apply to Major Leon A. Fox, Room 3050 Munitions Bldg., Washington.*] 6 x 9; ix + 71; 1933 (paper).

This doctoral thesis reviews the factors that have been suggested as responsible for evolution. The author concludes that mutations and chance variations do not furnish sufficient grist for the mill of natural selection and that purposeful adaptations must be postulated to account for the facts of evolution. He is inclined to interpret the inheritance of diabetes, refractive errors and glaucoma and the development of the trotting horse as examples of the inheritance of acquired characters. Whereas formerly it took years of training to develop a trotter, nowadays trotters are born, not made. There is a bibliography of four pages but no index.

THE LONG ROAD. *From Savagery to Civilization.*

By Fay-Cooper Cole. The Williams & Wilkins Co., Baltimore. $1.00. 5 x 7½; xi + 100; 1933.

Besides its display at Chicago of the tangible fruits of the past hundred years of scientific and technical development the Century of Progress Exposition is sponsoring a series of books presenting in brief our present-day knowledge of the various sciences. In this number of the series one of the leading American anthropologists tells the story of the long ascent from *Pithecanthropus* to modern man, the physical and cultural development that has taken place, and the differentiation of modern races. The book contains a bibliography of two pages, but no index. It is illustrated with reproductions of the admirable murals in Logan Museum, Beloit College, showing various stages of culture.

CONODONT STUDIES *Number 1 and Number 2. The University of Missouri Studies, Vol. VIII, Nos. 1 and 2.*

By E. B. Branson and M. G. Mehl. University of Missouri, Columbia. $1.25 each. 7⅞ x 10½; 167 + 12 plates; 1933 (paper).

Conodonts (fossil teeth supposedly from primitive fishes) are treated taxonomically in these studies to make them more useful as geologic horizon markers. A short introductory section gives methods of handling the material for study. Then follows the classification under subheads corresponding to sources of the material. Each section is supplemented with plates illustrating the species characters. The value of conodonts as horizon markers lies in the favorable possibilities for correlation in wide-spread forms, the short vertical range of species, and the hardiness of the material.

THE WORLD OF FOSSILS.

By Carroll L. Fenton. D. Appleton-Century Co., New York. $2.00. 4⅞ x 7⅜; x + 183; 1933.

A book that will interest the general reader. The author discusses fossil forms, from the earliest known creatures of the

water to mammoths and men, as the "remains of living organisms which had work to do, problems to solve and emergencies to meet . . . " The volume is illustrated with figures and maps, and contains a section on references and collections, a glossary, and an index.

GENERAL BIOLOGY

HVALRÅDETS SKRIFTER. *Scientific Results of Marine Biological Research. Nr. 7. Essays on Population.*

Edited by Universitetets Biologiske Laboratorium. Jacob Dybwad, Oslo. 7 x 10¾; 152 + 6 plates; 1933 (paper).

These studies emanate from the Biological Laboratory at Oslo, Norway. They are concerned with the report of investigations primarily related to the whaling industry. The task has been approached with intelligence, however, in that the authors have not hesitated to draw upon other forms for illustrative purposes as well as to delve into theoretical matters where needed. The present volume (the seventh in the series) is constituted by five separate essays. These are,

1) Johan Hjort: Whales and Whaling. 2) Per Ottestad: A mathematical method for the study of growth. 3) Alf Klem: On the growth of populations of yeast. 4) Johan Hjort, Gunnar Jahn and Per Ottestad: The optimum catch. 5) Norwegian Pelagic whaling in the Antarctic. II.

The first article on whales and whaling may be recommended as an interesting natural history report on the types of whales and their habitat relationships. The third paper on yeast growth attempts to analyze the general dependence of growth upon the means of subsistence, and the poisonous waste-products which develop with increase of population size. This paper, representing an abundance of experimental work, may well be called to the attention of yeast workers. In the essay on the "Optimum Catch" the primary problem of the volume is developed. Simply put, this problem is: how can the populations of the seas, specifically whale and fish populations, be exploited to a maximum degree and still not be in danger of extinction? The authors present data showing how some populations are affected by natural and human toll along with reserved propaganda for international rules and regulations on the whaling industry.

This book may well be drawn to the attention of biologists and represents a distinct contribution to the literature on population. The text seems not to have been carefully proof-read since various typographical errors occur including occasional misspellings. The essays are all in English.

THE UNIVERSE AND LIFE.

By H. S. Jennings. Yale University Press, New Haven. $1.50. 5⅛ x 8; 94; 1933.

This small volume represents a formal contribution to the philosophy of life and the universe. The book, to a biologically-minded reviewer at least, has the attribute of being clear and understandable when compared to the frequent and often enigmatical writings of the physical scientists. The subject matter is treated under the three following divisions: the nature of the universe as revealed by the study of biology, the nature of evolutionary progress, and the management of life. A fundamental thesis of the philosophy is that the universe, at first lifeless, later produces life through its own activities and with this production a new universe is created which," . . . begins to become conscious of itself, begins to feel, to think, to have ideas and purposes and ideals." In other words, in giving rise to mental processes new things arise in the universe which are only known as they occur and are experienced. The author also points out that life is moving onwards; following a new course towards a non-existing and non-predictable goal.

A stimulating book.

CYTOLOGICAL TECHNIQUE.

By John R. Baker. Methuen and Co., London. 3s. 6d. net. 4⅛ x 6¾; xi + 131; 1933.

Professor Baker has written briefly and

interestingly on the technique of making cytological preparations of animal cells, on the chemical and physiological properties of the important reagents and stains, and on the development of the standard techniques, and, everything considered, has been able to say a good deal in this little manual. As he puts it the

> guiding principle has been to give as few methods as possible, and to describe them in great detail, with full information as to the nature of the reagents used. It has been my object to tell the student throughout what he is doing, so far as is possible in such a difficult subject. All irrational methods, with which the literature abounds, have been carefully avoided. Many investigators have published formulae for fixatives and other fluids, without giving any indication of the researches which have led up to them. They have given no concrete evidence of why they are to be preferred to other fluids, nor of whether various proportions of the substances used were tried, with what results.

The use of this handbook ought to lead to more critical use of cytological methods on the part of students and it might serve to stimulate research on the methods themselves by more advanced workers.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 413. Allgemeine vergleichende Physiologie.* Containing following articles: *Das nasse photographische Kollodiumverfahren im Dienste des Biologen*, by Carl J. Cori; *Methodik der biologischen Lichtwirkungen*, by Ludwig Pincussen; *Über die Methoden zur Konstanthaltung von Temperatur und Luftfeuchtigkeit im biologischen Laboratoriumsversuch*, by Ernst Janisch.

Urban und Schwarzenberg, Berlin. 5.80 marks. 7 x 10; 112; 1933 (paper).

Cori claims a greater sharpness of photographic detail and less expense for a moist collodion plate method used in commercial photography, and describes his method for preparing and handling the plates.

Pincussen covers several phases of the new developments in his subject; his descriptions of equipment include the newer types of light sources whose radiation is limited to narrow spectral bands, thermocouples, and photoelectric cells. He devotes about 30 pages to methods of studying light reactions of plants and animals. Lunz contributes a chapter on a large spectroscope with two prisms for producing monochromatic light.

Janisch discusses the elementary principles that ought to govern the selection and care of constant temperature apparatus describing little niceties of technique. He takes up some simple methods of humidity control that make use of the vapor pressure of known concentrations of salt solutions, presenting tables showing the relative humidity of air in equilibrium with certain solutions at different temperatures.

ÜBER ENTSTEHUNG UND ENTWICKLUNG DES LEBENS.

By Fr. Šamberger. Leopold Voss, Leipzig. 2.40 marks. 5 x 7⅞; 55; 1933 (paper).

The author, director of the dermatological clinic of the University of Prag, sets forth in this booklet a theory of evolution based on an alleged principle that higher organisms, including man, developed from lower forms of their own special protoplasm and not from lower *organisms*, as assumed by the current theories. Accordingly, all life has passed first through its "premiere" of evolution in which the original types of all individuals developed from the specific original cells of their genus. Man, therefore, he concludes, was formed not through phylogeny but through anthropogeny, as witnessed by the naevi and all atavisms in man. The author sees the source of the whole development of all living matter in an inner striving after the optimum of existence; the evolutionary transformations of living matter flow from its own initiative activity and are by no means the mere consequence of the passive adaptability to outer stimuli, as assumed by Lamarck, Darwin and Roux. The book lacks index and bibliography. [Reginald, our procacious Office Boy, says that the only biologists likely to be influenced by this treatise are God and Bernard Shaw.]

JUNGLE MEMORIES.

By Henry H. Rusby. Whittlesey House, McGraw-Hill Book Co., New York. $3.50. 5¾ x 9; xiii + 388; 1933.

Professor Rusby recounts the story of his expedition in 1885–87 to South America in search of coca and other medicinal plants. Across the Andes, through the jungles of the Beni, Madeira and Amazon valleys he traveled using the most primitive means of transportation. The strength of his personality is evident in every page, as in a nonchalant manner he briefly relates some mortal peril evaded or difficulty overcome. His almost daily adventures did not deflect him from the scientific objective of his expedition and he continuously noted and collected plants and animals. A description of the flora and fauna of those regions he has given in his scientific publications but what is mentioned here is sufficient to emphasize the richness of life there. We suggest that this book should be read in connection with that classic of Amazonian travel literature, *White Waters and Black*.

ELEMENTS OF BIOLOGY *With Special Reference to Their Rôle in the Lives of Animals.* *By James W. Buchanan. Harper and Brothers, New York.* $3.00. 5⅜ x 8; xx + 478, 1933.

This textbook has been designed with the liberal arts student in mind: "the content of the volume includes the understanding and information that the intelligent layman might reasonably be expected to have as the result of his experience in a general course." The arrangement follows in the main the conventional type of general biology textbooks but more references to man are included and a greater use of small mammals as laboratory material is suggested. The book concludes with a glossary and an adequate index, and suggested reading lists are given at the end of each chapter. The illustrations are for the most part new or redrawn.

THE ASSOCIATION OF THE TERMITES, KALOTERMES MINOR, RETICULITERMES HESPERUS, AND ZOOTERMOPSIS ANGUSTICOLLIS WITH FUNGI. *University of California Publications in Zoology, Vol. 39, No. 5.* *By Esther C. Hendee. University of California Press, Berkeley.* 30 cents. 6¾ x 10½; 24; 1933 (paper).

Representatives of 33 genera of fungi were isolated from the colonies of three species of termites of diverse habitat. No fungus appeared to be peculiar to any one termite species, and *Penicillium* and *Trichoderma* were isolated from each. Neither was there any specificity as far as the wood was concerned, and it appears that the termites carry spores about with them. No decision was reached as to whether the fungus makes wood more available or more attractive to termites.

THE BIOLOGY OF THE PROTOZOA. *Second Edition. Thoroughly Revised.* *By Gary N. Calkins. Lea and Febiger, Philadelphia.* $7.50. 5¾ x 9½; xi + 607; 1933.

Previous mention has been made in these pages of the first edition of this very excellent standard textbook. The present edition has been thoroughly revised and rearranged. It emphasises the conception of a changing organization brought about by continued metabolism. This conception "furnishes the basis for an interpretation not only of life histories but of the significant biological phenomena of cell division, maturity, sex differentiation, fertilization and senescence as well." There are new sections on parasitism and disease. Reference to protozoan flagellates has been limited to the Zoömastigophora. The bibliography has been brought up to date.

NEW INTRODUCTION TO BIOLOGY. *By Alfred C. Kinsey. J. B. Lippincott Co., Chicago.* $1.68. 5¼ x 7½; xxiii + 840; 1933.

The review of the first edition (noticed in Volume 2 of this REVIEW) considered it particularly noteworthy because of its excellent illustrations. The second edition has even more and equally good illustrations. The arrangement of the various main subjects has been changed somewhat and new material has been added. Most

of the references are at the end of the sections to which they refer. Summaries, problems, and a glossary should make it a useful high school text. There is a good index.

LIFE-GIVING LIGHT. *A Century of Progress Series.*

By Charles Sheard. The Williams & Wilkins Co., Baltimore. $1.00. 5 x 7½; x + 174; 1933.

Sheard's book is plentifully studded with quotations from scientific textbooks, classical literature, and the Bible, and its style frequently rises to such heights as, "Man, a creature of heaven-given light, has supplemented Nature by first imitating her and then, having wrested some of her secrets from her, by challenging her." With the aid of this little book the whole subject of the biological effects of light can be made plain to any clergyman.

BÉCHAMP OR PASTEUR? *A Lost Chapter in the History of Biology.*

By E. Douglas Hume. Founded upon MS. by Montague R. Leverson. C. W. Daniel Co., London. 6 shillings. 5¼ x 8; 301; 1932.

This is the second edition of a bit of anti-vivisection and anti-vaccination propaganda intended to prove that Pasteur's conclusions are false and that one of his opponents, Béchamp, ought to be regarded as a hero by all health faddists. Maybe he should.

THE LIVING UNIVERSE.

By Sir Francis Younghusband. E. P. Dutton and Co., New York. $3.00. 5⅜ x 8⅓; x + 252; 1933.

In this book the author develops further his interpretation of the universe as the manifestation of a Cosmic Spirit and his speculations as to the existence of life on the planets of other stars.

BULLETIN DE L'ASSOCIATION DES DIPLÔMÉS DE MICROBIOLOGIE DE LA FACULTÉ DE PHARMACIE DE NANCY, No. 6.

Faculté de Pharmacie, Nancy. 6¼ x 9⅜; 48; 1933 (paper).

HUMAN BIOLOGY

JOCASTA'S CRIME. *An Anthropological Study.*

By Lord Raglan. E. P. Dutton and Co., New York. $2.25. 4⅞ x 7¼; xii + 215; no date.

Few acts are surrounded with such an aura of horror as incest, yet we should be hard put to explain why intercourse between near kin should arouse a reaction of horror. Nor have moralists or anthropologists been more successful in their attempted explanation of the incest taboo. Some have postulated an instinctive basis; but an instinct which somehow got left out of the composition of the ancient Egyptians, Persians and Incas is hard to credit. Some have derived the incest taboo from observation of the harmful effects of inbreeding; but there is no good evidence that inbreeding is harmful and, as Lord Raglan points out, primitive man was not a legislator who established new customs to serve certain rational ends.

Lord Raglan himself suggests that the origin, not only of the incest taboo, but of the taboos on menstruous women and on a man's meeting his mother-in-law, is to be found in magic. The second of these taboos is probably the oldest.

> We may perhaps suppose that the mysterious and apparently causeless character of the [menstrual] flow filled early man with a surprise which developed gradually into alarm, and later into horror, and that he developed *pari passu* a most elaborate system of precautions against the magical dangers which he believed to threaten him.

Since running water is a sovereign antidote against magic, the safest plan was for the men of a tribe to live on the opposite side of a stream from the women and merely visit them surreptitiously for intercourse.

> This would not last for very long; old men would stay with the women, and old women with the men, and the idea might gradually arise that it was safe for people of both sexes to live on the same side so long as they did not have sexual intercourse. There was as yet no individual marriage—the divine king and queen

were the first pair to be married—but the men would tend to visit the same women, those nearest to them on the opposite bank, and these would often be in fact their cross-cousins, though the relationship was not yet recognized: when individual marriage came into existence, cross-cousins were the people between whom it could be most easily arranged, if the rule of exogamy was to be observed.

All this time, we may suppose, the belief in ancestral spirits had been slowly developing, and the danger from a breach of the menstrual taboo, which had formerly been supposed to come from the women themselves, or their blood, was in part transferred to the spirits, who thus became jealous guardians of the taboo. The women were secluded during their period, and old women were set to guard them. .... Eventually, when individual marriage was evolved, the individual mother-in-law replaced the old women of the group.

In this way would come about the organization of the tribe into exogamous clans, the members of which would be forbidden to have intercourse with each other, however remote their relationship might be, while near relatives might have intercourse if they belonged to different clans. In a later stage of development the clan system broke down and the taboo on intercourse was applied to near relatives on both the father's and mother's side.

There are, however, also traces of another magical system, a creation ritual in which a brother married his sister. The myth of Oedipus and Jocasta is, according to the author, a reminiscence of the conflict between these two incompatible systems of magic, one commanding a man to marry his sister, the other forbidding him to do so.

This is an extremely interesting book on a puzzling subject. The author does not claim to have proved his theory; it has, however, the advantage over previous theories that it attempts to account for all the features of the three early sexual taboos and not merely a few selected features of one selected taboo. The author has been so thoughtful as to provide the reader with a bibliography of six pages and an excellent index.

POPULATION TRENDS IN THE UNITED STATES. *By Warren S. Thompson and P. K. Whelpton. McGraw-Hill Book Co., New York.* $4.00. $5\frac{7}{8}$ x 9; x + 415; 1933.

When Mr. Hoover's Research Committee on Social Trends was preparing its report, it was found that the results obtained in a number of the investigations required more extended treatment than could be given in the report itself. In addition, therefore, to the summaries of these subjects in the report separate monographs are published giving the detailed evidence and discussion. In this monograph the authors deal with the growth and distribution of population in the United States, the national origins of the white population, age and sex composition, marital condition, birth and death rates, population growth from immigration and natural increase, probable trends and consequences of future growth, and population policy. Since 1920 the growth of most agricultural states has been slow as compared with earlier decades. In metropolitan districts the growth of the suburbs has been much more rapid from 1920 to 1930 than that of the central cities. The forces affecting the growth of particular localities and the probable effects of national planning are discussed.

In administering the quota provisions of the immigration laws it was necessary to estimate the national origins of the white population in 1920. According to the estimates of the committee 41.4 per cent was of British and North Irish origin, 16.3 per cent German, 11.2 per cent South Irish, 4.3 per cent Scandinavian, 5.8 per cent from other countries of northwestern Europe, and 4.3 per cent Canadian. Thus about 83 per cent of the white population had its origin in northwest Europe or Canada. The high birth rates of the newer immigrants rapidly decline, while the rural population, which is of predominantly northwestern European origin, has a higher birth rate than the urban. It is therefore probable that the composition of the population will not change materially. However, the authors see no eugenic basis for the present quotas and suggest that it may be desirable "to encourage a moderate number of superior persons having the background of other cultures to migrate hither, to the end that our social order may keep itself fluid and adaptable."

On the basis of extrapolated trends in birth rates, death rates and immigration, the authors derive various estimates of the

future growth of the population of the United States. According to the highest of these the population will exceed 200,000,000 in 1980 with no indication of growth ceasing for several decades thereafter; according to the lowest it will increase to about 136,000,000 in 1955 and then decrease to about 126,000,000 in 1980; according to the trends which the authors consider most probable the population in 1980 will be almost at its maximum size with about 155,000,000 persons.

The final chapter deals with the change from the former encouragement of immigration and fecundity to the present restriction of immigration and largely unintentional but nevertheless effective handicaps on the production of large families. If an increase of the birth rate is felt to be desirable, the authors suggest mothers' wages as a means of encouraging it. As to the program of the eugenists, they point out some of the difficulties of agreement as to who are "desirable" and "undesirable." Besides the numerous tables in the text there is a tabular appendix of 70 pages and an index.

AMERICAN WINES *and How to Make Them. By Philip M. Wagner. Alfred A. Knopf, New York.* $2.00. 5¼ x 7½; xix + 295; 1933.

Mr. Wagner started out to be a crusader, aiming to lead his people out of arid America into a promised land flowing with good wine grown if not in the back-yard at least on the nearby farms of the land of the free. But fate, this time in the persons of an oddly assorted pair, Capt. W. H. Stayton and Franklin D. Roosevelt, played this gallant young knight a dirty trick. Repeal beat him to it, as the phrase goes. Wine flows in this so lately dry land, right enough. But even now only an infinitesimal fraction of it is home-made wine, and in six months still less of it will be. In short, we greatly fear that noble as was Mr. Wagner's intent the fact is that no sensible man who has anything else to do is going to make wine at home when he can buy a good product at a moderate to low price. The reason is simple, and is really better and more clearly stated in this excellent book than anywhere else we can recall. It is that to make a really good wine requires a lot of care, thought, intelligence, labor, and trouble. To make a distinguished wine requires in addition God's active coöperation, and experience shows that this coöperation, in the matter of wine making as well as others that might be mentioned, is distributed with a certain discrimination. To be sure our author urges that it is *not* much trouble to make good wine at home. But what constitutes "much trouble" is plainly a relative matter. If you can only buy good wine surreptitiously, uncertainly, and at relatively great cost, then the trouble required to make it is worth taking. But if, on the other hand, you can buy it from the chain store around the corner at less than a dollar a bottle the case becomes distinctly otherwise.

It pains us uncommonly to take so realistic an attitude, because Mr. Wagner has put an enormous amount of research into the writing of this book, and altogether has done a fine piece of work. It is just too bad that it will not have the influence that it would have had if Bishop Cannon had continued to rule the land.

THE PENINSULA OF YUCATAN: *Medical, Biological, Meteorological and Sociological Studies. Carnegie Institution of Washington Publication No. 431.*

By George C. Shattuck. In collaboration with the following authors: Joseph C. Bequaert, Francis G. Benedict, William J. Clench, Frank H. Connell, Kenneth Goodner, Margaret M. Hilferty, Helen C. Maher, Katheryn McKay, John L. Page, Robert Redfield, Oliver Ricketson, Jr., Jack H. Sandground, George M. Saunders. Carnegie Institution of Washington. $8.00 (paper); $9.00 (cloth). 9 x 12; xvii + 576 + 68 plates + 2 folding maps; 1933.

Thirteen collaborators assisted the author in producing this interesting and useful volume. The three expeditions, organized by the Department of Tropical Medicine of Harvard University under the auspices of the Carnegie Institution of Washington, were made in 1929, 1930 and 1931. The first part of the volume (chap-

ters I–V inclusive) deals with general and miscellaneous information about Yucatan. This part will be most useful to those wishing to visit Yucatan either for purposes of investigation or for travel. In part II (chapters VI–XXI inclusive) will be found reports of medical surveys and other data. Much detailed information is given also concerning local living conditions in Guatemala and British Honduras and among the Mayas of Chiapas. There are sections on bacteriological and serological studies; certain helminthic and protozoan parasites of man and animals; tests on basal metabolism, Kahn tests and blood counts; syphilis and other diseases; vital statistics and meteorological studies. In the concluding chapter of part II is given a general survey of the more important facts brought out by the data presented, a discussion of the opportunity for further studies of disease and the necessity for prolonged and coordinated investigations of the problems involved in the adjustment of various races to tropical climates. Part III (chapters XXII–XXVI inclusive) deals with the surveys in 1931 which were concerned chiefly with malaria and amoebiasis. Data are presented also on observations in blood-pressures, blood studies and other clinical material.

The fourth and last part (chapters XXVII–XXIX inclusive) is concerned with the natural history of the region of Chichen Itza. The volume is well documented and is abundantly illustrated with excellent plates, maps and drawings. There is no index, an inexcusable omission in such a work.

PROMINENT DANISH SCIENTISTS *Through the Ages. With Facsimiles from Their Works.*

By V. Meisen, Editor. Levin and Munksgaard, Copenhagen. 9 Dan. Cr. 7¾ x 11; 195; 1932 (paper).

This is an entertaining book. Issued in commemoration of the 450th anniversary of the University Library of Copenhagen it gives a condensed survey of the achievements of Danish men of science in those studies which the library is particularly intended to serve: namely, natural science, mathematics and medicine. The forty-five men in review (none is now living) were selected on the basis of their original discoveries or theories of far reaching significance. One of the most fascinating features of the book is the reproduction, in facsimile, in each essay, of characteristic fragments of the man's work. Unfortunately no portraits are given. Among the first of those listed are Henrik Harpestraeng (d. 1244) the earliest Danish medical author whose most famous writings were on herbs. Then comes Petrus de Dacia (1300) renowned for his calendar and astronomical tables. From the middle of the seventeenth century beginning with the two Bartholins, Thomas (d. 1680), anatomist, and Erasmus (d. 1698) physicist, Oluf Borch (d. 1690) chemist and botanist and Nicolaus Steno (d. 1686), one of the greatest Danish naturalists, the number of investigators increases much more rapidly. Considerably later comes Otto Fabricius who when a young man went to Greenland (1768–73) as a missionary. His *Fauna of Greenland*, written in Latin, describes 473 animals, 130 of which were new to science. Johann C. Fabricius, who did his pioneering work in insects, was of the same period.

Classifying these famous men by professions we find 7 anatomists and physiologists, 6 botanists, 11 zoologists and biologists, 18 physicians, 4 geologists and paleontologists, 14 chemists, 4 physicists, 3 mathematicians, and 3 astronomers. The volume is indexed.

OUT OF MY LIFE AND THOUGHT. *An Autobiography.*

By Albert Schweitzer. Translated by C. T. Campion. Henry Holt and Co., New York. $2.50. 5⅜ x 8¼; 288; 1933.

Dr. Albert Schweitzer is one of the most amazing and romantic figures of the modern world. Born an Alsatian he became in turn, but without relinquishing activity in the old fields as he took on new ones, a university teacher and researcher in philosophy, a great organist, one of the world's leading authorities on Bach and his music, and a medical missionary in Equatorial Africa. All of this has been packed into the 58 years since he was born. In an

earlier number of this REVIEW (Vol. VII, pp. 227–8) there appeared a notice of his *On the Edge of the Primeval Forest*, a most interesting and stimulating account of his medical work in Africa.

The present book tells as much of his whole life story as he is apparently willing to put on paper. Those readers who are not deeply religious, as he is, will regret that it is obviously his nature to speak much more freely and fully about his inner spiritual life than about his material doings. He is plainly a vigorous, motor, cyclothymic personality type, and has had an extraordinary range of interesting experiences. But he tends to be reticent about them, and particularly so at crucially interesting points. On the other hand, he expounds his theological, ethical and religious ideas at great length. However, in spite of this defect, if it is a defect, this is a notable autobiography. We recommend it to our readers.

AMERICAN AGRICULTURAL VILLAGES: 1930. *An Analysis of Census Data. American Statistical Association Monograph No. 1.* *By Irving Lorge under the direction of Edmund deS. Brunner. American Statistical Assoc., New York.* $1.00. 6 x 9¼; iii + 133; 1933 (paper).

This is a continuation for the Census of 1930 of the work of Fry in *A Census Analysis of American Agricultural Villages* for the Census of 1920. It deals with the same 177 villages, for each of which is given the population growth or decline from 1920 to 1930, as well as population by color, nativity and parentage, native whites born in or outside of state of residence, place of birth and year of immigration of foreign-born, age distribution by sex, marital condition by sex, school attendance by sex and age, illiteracy by sex, color and nativity, naturalization of foreign-born whites, home ownership, size of family, gainfully employed persons by sex and age, occupations, and marital condition of gainfully employed females. Totals are also presented for the Middle Atlantic states, the South, the Middle West, the Far West and the United States. The interpretation of the tables is part of a report of President Hoover's Research Committee on Social Trends. It is, however, noted that

> In 1930 agricultural villagers represent an older population, a larger proportion of which is married. The population is more literate and attends school to a considerably greater degree than was the case in 1920. Occupationally, they find principal employment in four major industry categories. Manufacturing, trade or merchandising, agriculture and transportation, in that order. Of these, merchandising made the largest proportional gain. There are fewer very young and very old males gainfully employed. In general, however, there has been a consistent increase in the number of gainfully employed females except in the early age brackets.

TO BE OR NOT TO BE. *A Study of Suicide. By Louis I. Dublin and Bessie Bunzel. Harrison Smith and Robert Haas, New York.* $3.50. 5¾ x 8½; x + 443; 1933.

In this interesting study Dr. Dublin deals with suicide in its statistical, ethnological and psychiatric aspects. For his statistical treatment he draws not only from the official publications of various countries but also from the records of the Metropolitan Life Insurance Company. The differing tendency towards suicide in different races is, he concludes, the result of social rather than of biological heritage. There is a correlation of $-.47 \pm .05$ between the business index and the suicide rate corrected for trend and seasonal variation in ten large American cities from 1910 to 1931. The lower suicide rates in Catholic countries, in rural areas and among married persons he attributes to the greater social integration of these groups.

In his ethnological section Dublin shows that the horror of suicide characteristic of the Hebrew and Christian tradition is by no means universal. Among some primitive peoples suicide is unknown, while in others it is quite prevalent. The religions of the Far East in general approve of it, and it is even institutionalized in the customs of suttee and hara-kiri. The Greeks and Romans took a much more lenient view of suicide than do most modern moralists. Nor was it until the time of Augustine that the Christian church regarded it as a crime under all circumstances.

In his psychiatric section Dublin shows

the value of psychoanalysis in understanding the motives behind suicide—emotions of fear, inferiority, hatred and guilt—and the influence of mental disease on the act. He also considers methods which have been tried for preventing suicide and the broader movement towards sound mental health for the population as a whole. There is an appendix of statistical data, a bibliography of 14 pages and an index.

EMPLOYED BOYS AND GIRLS IN ROCHESTER AND UTICA, NEW YORK. *United States Department of Labor, Children's Bureau Publication No. 218.*

By Alice Channing. U. S. Government Printing Office, Washington. 10 cents. 5⅞ x 9⅛; v + 74; 1933 (paper).

Serious students of sociology have long held the theory that children who are either too dumb to learn, or whose parents cannot afford to permit the continuance of their education, or who for other reasons are fed up on schooling, tend to leave school and go to work as soon as it is legally possible to do so. This treatise proves that this theory was correct so far as concerned Rochester and Utica, N. Y., in the late spring of 1927. The proof is thorough, precise, documented, and detailed. For ourselves we shall never doubt the theory again, at least so far as concerns Rochester and Utica, although (*vide* Table 2, p. 5) there was one employed girl in Utica whose age was not ascertained at the time of the study. The report is packed with information of thrilling interest, novelty, and importance. For example it is shown by the data of Table 13 (p. 45) (in more detail than we can spare space for here) that the median cash wage of 16 year old boys who had been employed one year or *more* was higher by $2 per week than that of 16 year old boys who had been employed *less* than one year. To give another example, from the many presented: careful analysis of the intelligence tests demonstrated that the higher the boys' I.Q.s the less often were they out of a job. Who, we ask you, would ever have supposed *that* to be true? The same relationship, we are shocked to report, was found *not* to hold for the girls. [Reginald the Office Boy, whose worldly wisdom is becoming appalling, says that he knows why.]

We regret that lack of space forbids any more extended review of this profound and thorough piece of research.

A GERMAN DOCTOR AT THE FRONT. (*Die Front der Arzte.*)

By Wilhelm His. Translated from the Original German by Gustavus M. Blech and Jefferson R. Kean. National Service Publishing Co., Washington. $2.50. 5⅞ x 9; viii + 230; 1933.

The discoverer of "the bundle of His" and the son of the renowned anatomist, also named Wilhelm, records here his experiences in the Great War during which he held one of the highest medical positions among the Germans, taking him into the various theaters of the war on both eastern and western fronts. He describes what he has seen simply and dispassionately, uncolored by any sentiment of his own. The Great War he regards not as the greatest catastrophe in the history of mankind, as some assert, but rather as the greatest of human experiences. The medical lessons which the war gave in the care and restoration of the injured, in the shelter, feeding, moving and care of masses under primitive conditions, that is, the control of epidemics, he thinks will last. The progress of medical science between 1850–1920 is portrayed in the table (p. 228) giving the ratio of killed to died from disease:

In the war in the Crimea 1854–56 (English)	1. to 3.6
In the Italian war 1859	1. to 3.3
In the Austro-Prussian war 1866 (Prussians)	1 to 1.3
In the Franco-Prussian war 1870–71	1 to 0.53
In the World War (Germans)	1 to 0.1

The book should be of interest to military medical men and physicians and surgeons, and possibly also to the general reader who may enjoy the descriptions of the various countries and their populations, and frequently interspersed sidelights of human contacts which relieve an otherwise dark background.

THE BARBARY COAST. *An Informal History of the San Francisco Underworld.*

By Herbert Asbury. Alfred A. Knopf, New York. $3.00. 5⅝ x 8⅛; 319 + xi; 1933.

This history of low life in San Francisco is detailed, realistic and meticulously documented. The life stories of such colorful ladies as the Galloping Cow, who tended bar in her own saloon and once broke a man's back because he had ventured to chuck her under the chin, and Tessie Wall, who drank twenty-two bottles of wine "without once leaving the table" at her first dinner with the man she subsequently married, and still more subsequently shot, are told in all simplicity and candor. Tessie, by the way, only retired from business in the year 1918 or thereabouts. And this fact is of significance in the appraisal of the whole story which Mr. Asbury sets forth with such great skill and erudition. It is not ancient history, not something comparable to the doings of the Caesars as recorded by Suetonius, but an account of the goings-on in this country within the life time of probably a majority of the readers of this REVIEW. As we have had occasion to remark before, man is an oddly curious animal.

The book is abundantly and entertainingly illustrated, has a bibliography and an excellent index.

AN INTRODUCTION TO THE SOCIOLOGY OF ISLAM. *In Two Volumes.*

By Reuben Levy. Williams and Norgate, London. £1.1.0 per volume. Vol. I. 5⅜ x 8⅝; viii + 410; 1931. Vol. II: 5⅜ x 8⅝; v + 426; 1933.

Although the author covers a wide field in these two volumes—grades of society, status of women and children, jurisprudence, Caliphate and central government in Islam in the first volume, and religious conceptions, moral sentiments, usage, custom and secular law, government in the provinces, military organization and science in the second—he has succeeded in giving an extremely lucid account of a system of religion and government which not only has been of great importance in the history and politics of the past in the East, but still is at the present time. The author considers the theories of the classical period of Islam, but also makes an attempt "to discover how far the people who have accepted Mohammedanism are content to be guided by them;" how they have adapted the laws as embodied in the Koran according to local custom or tribal law, and how religious difficulties have been overcome with the introduction of a Western system of popular government in Turkey. Like most peoples the followers of Islam have evolved ways of "getting around the law."

Besides numerous annotations, each chapter is equipped with a bibliography, and each volume its own index. A thoroughly scholarly and important treatise.

HEART BURIAL.

By Charles A. Bradford. George Allen and Unwin, London. 8s. 6d. net. 5¼ x 7½; 256; 1933.

It is the author's opinion that heart burial "was but an offshoot of the practise of evisceration at death which had been in vogue from the later Stone Age." It appears that the desire to check bodily decay led to the separate disposal of the viscera wherever the custom of embalming was in use, though in Egypt it was eventually done with special pomp and ceremony. In occidental Europe the first authentic record is that of the separate burial of the heart of D'Arbrissel, founder of the order of Fontrevault, in 1117 A.D. The author agrees in the opinion that it was due to "feminine tenderness tinged with religion."

Since the twelfth century, in the area that is now the London Postal Area there have been recorded 108 cases of separate burial of the viscera at death. Of these "some 55" are authentic cases of heart burial. The first mentioned is that of King Henry II in 1189, and the last that of Thomas Hardy in 1928. The cases are listed chronologically and alphabetically but no attempt is made to give a reason why in these particular cases it should have been done.

ERINNERUNGEN, *Bekenntnisse und Betrachtungen.*

By Gottlieb Haberlandt. Julius Springer, Berlin. 9.60 RM (paper); 10.80 RM (cloth). 5½ x 8½; vii + 243, 1933.

Two features of this autobiography of the author of *Eine Botanische Tropenreise* and *Physiological Plant Anatomy* deserve mention here. One of them is the air of *Gemütlichkeit* that pervades the book and which only partly comes from the anecdotes that turn up every few pages. Haberlandt writes more about his teachers and colleagues and the universities in which they worked than he does about himself. During his student days at Vienna and at Tübingen and during the rest of his career at Graz and Berlin he worked with the men who helped to make German universities the powerful influences they were, and his notes on the men and the period are valuable in themselves. The other characteristic of the book is his effort to account for the influences of family life and education that turned him toward a scientific career and to outline the train of thought that led him from one scientific problem to another. Haberlandt is an interesting man and his own interests and accomplishments are not confined to academic subjects.

APPLIED EUGENICS.

By Paul Popenoe and Roswell H. Johnson. The Macmillan Co., New York. $2.60. 5⅛ x 7¾; ix + 429; 1933.

The authors here present a revised and augmented edition of this standard work on eugenics which was first published in 1918. Its principal object is to propagate the tenets of orthodox eugenics and the authors employ their wide knowledge of biology and sociology in the attempt to give a scientific basis to these doctrines.

The mechanism of inheritance and the importance of the hereditary factors are well summarized in the light of modern genetics. They emphasize the burden placed on society by the inferior individuals and so demonstrate the need to eliminate these and increase the number of those of superior caliber. The measures proposed in order to achieve this are well known: segregation and sterilization of the feeble-minded, more stringent marriage laws, inducement, financial or otherwise, to superior individuals that they marry earlier and reproduce more, etc. All this, they foresee, will ameliorate the society of the future because, "the best then will be better than the best now and the worst then be better than the worst now."

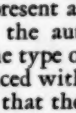
The philosophy underlying this work may or may not be acceptable to the reader, but notwithstanding, this book remains valuable as a source of literature for the student in the field of human biology.

THE PUBLIC DOMAIN OF NEVADA AND FACTORS AFFECTING ITS USE. *U. S. Department of Agriculture Technical Bulletin No. 301.*

By E. O. Wooton. U. S. Government Printing Office, Washington. 75 cents. 5⅞ x 9⅛; 52 + 2 folding maps; 1933 (paper).

The necessity of some legislative action to regulate the use or disposal of the public domain has been and is continually urged. In those states where these lands are extensive it becomes a complex economic problem. Especially is it so in Nevada where they constitute about 95 per cent of the area of the state and the whole agricultural organization is here dependent upon their use, present and future.

This problem the author investigates with regard to the type of legislation that would be introduced with greatest benefit to all. He finds that the physical peculiarities of the different regions of Nevada added to the desire of the established stockmen to maintain the *status quo*, or at least lose nothing by a change, complicate the question in this state more than elsewhere. With all those who have written on the subject he advocates that something be done about it, but, confronted by the above difficulties, proposes that a more intensive study be undertaken before an attempt is made to enact laws.



MEXICO BEFORE CORTEZ. *An Account of the Daily Life, Religion, and Ritual of the Aztecs and Kindred Peoples.*

By J. Eric Thompson. Charles Scribner's

Sons, New York. $2.50. 5½ x 8; x + 298; 1933.

The layman who reads this book will be amazed at the extent of information concerning the racial cultures of early Mexico which diligent research has uncovered. The author, in charge of Central and South American archaeology at the Field Museum of Chicago, has written an authoritative and highly readable account of the daily life, religion and ritual of the people who inhabited the Valley of Mexico and adjacent regions before the time of Cortez. The volume is to a certain extent complementary to T. A. Joyce's *Mexican Archeology*. Training of children was very strict. Separate colleges existed for educating the sons of the middle classes and of the nobility but both types of school were attached to religious temples so that the training could be closely supervised by the priests. The habit of lying had little hold on these peoples, not because they were inherently honest, but because of the severity of the punishment meted out to the offender, particularly to the young. The account is an amazing record of a people, primitive yet civilized, intensely superstitious, highly skilled in artistry and possessing a remarkable code of ethics. The volume is interestingly illustrated and is indexed.

THE POPULATION OF ATHENS IN THE FIFTH AND FOURTH CENTURIES B.C.

By A. W. Gomme. Basil Blackwell, Oxford. 5 shillings net. 5⅜ x 8⅜; vii + 87 + folding map; 1933.

On the basis of statements of army strength in Thucydides and other historians, of numbers of recruits in inscriptions, of definite but not always trustworthy statements about the total adult male population, and, as a check, of the production and importation of grain, the author makes a rough estimate of 315,500 for the total population of Attica in 431 B.C., which was reduced by the plague to 218,000 in 425 and again rose to 258,000 by 323. Between 480 and 431 there had apparently been a considerable natural increase in the population. The number of slaves, he concludes, was at no time larger than 100,000 to 120,000. He also finds no reason to suppose that the exposure of children was a regular practice. The evidence for this, he points out, is mostly drawn from the romantic plots of tragedies and comedies and "the motive is never economic, but a warning oracle from on high, or because the child is illegitimate—a reason which has caused infanticide or exposure in all times and countries."

GREAT MEN OF SCIENCE. *A History of Scientific Progress.*

By Philipp Lenard. Translated from the Second German Edition by H. Stafford Hatfield. The Macmillan Co., New York. $3.00. 5¾ x 8½; xx + 389; 1933.

This is a useful book to have available, particularly for graduate students in the sciences, and also, to speak euphemistically, to enable their professors to "refresh their memories" about the lives and doings of their intellectual forbears. It is composed of brief, but adequate, accounts of the lives, principal achievements, and significance of over 60 of the great figures in the history of science, beginning with Pythagoras of Samos and ending with Friedrich Hasenöhrl. The author is a Nobel laureate in physics, and it is therefore natural that the men treated are preponderantly from the physical and mathematical sides. Indeed Charles Darwin and Linnaeus are the only primarily biological persons who get a separate (shared) chapter. In this chapter, however, is included some account of another biologist called, oddly to our eyes, Johann Mendel.

The treatment throughout is highly intelligent, readable and critical. The book is an important addition to the reading literature of scientific history. It has name and subject indices.

DIE BEHAARUNG DES MENSCHEN. *Eine sexual- und konstitutions-wissenschaftliche Abhandlung. Monographien zur Frauenkunde und Konstitutionsforschung Nr. 17.*

By Oskar F. Scheuer. Curt Kabitzsch, Leipzig. 6 marks. 6¾ x 9⅝; ii + 93; 1933 (paper).

Within 87 pages, the author covers the subject of human hair. He goes all the way from the scent of hair to the bearded ladies of the side-shows. Man's relation to other mammals in respect to hair, racial differences in hair, baldness, physiological functions of hair, head hair, pubic hair, axillary hair, beards in male and female, extent and distribution of hair in the two sexes,—all these and many more aspects are discussed. Nearly 4 pages are devoted to Darwin's theory of the sexual selection of hair and the various opinions advanced since to replace it.

The paper is a collection of the opinions and observations of a good many different investigators and writers. There are 17 illustrations, many of them—such as the picture of Zenora Pastrana, the bearded lady—being the same ones that usually appear in treatises on hair. The bibliography includes 175 titles, nearly all of which are German.

THE LAND OF FEAST AND FAMINE.

By Helge Ingstad. Translated from the Norwegian for the first time by Eugene Gay-Tifft. Alfred A. Knopf, New York. $3.50. 5⅝ x 8¼; [11] + 332; 1933.

An interesting account of life in the Barren Lands of Canada by a Norwegian attorney, who tired of the placid life of the law and became a hunter and trapper. It contains a wealth of shrewd observations on the Indians of the far North, and on the habits of the game and furbearing animals. And the story has plenty of thrills. The author lived off the country and had no resources other than his own to fall back on. About the only thing in the whole book (which contains some tall tales) that at first stirred a certain low scepticism within us was the alleged scorn with which he successfully rebuffed the amorous advances of the ladies of those sparsely settled parts. But in the end we concluded that this was probably all right; the ladies in question appear to have been long on dirt and short on pulchritude. The book contains some excellent photographs, taken by the author. There is no index.

LIFE OF CHRISTIAN SAMUEL HAHNEMANN. *Founder of Homœopathy.*

By Rosa W. Hobhouse. C. W. Daniel Co., London. 7s. 6 d. net. 5¼ x 8; 288; 1933.

Although exhaustive treatises on the life and work of Hahnemann are well known to the professional medical historian and brief biographical sketches of this great pioneer are readily accessible in the shorter medical histories, no easily available short survey of Hahnemann's life and contributions to medicine has been provided for the casually interested student of medical history. Mrs. Hobhouse, with a fine insight into the kind of biography that would supply this need, has presented in this inexpensive little book an interesting and well written account of the life of this extraordinary doctor. A short list of Hahnemann's most important writings, a list of books about Hahnemann, and an adequate index are included.

RACES AND ETHNIC GROUPS IN AMERICAN LIFE.

By T. J. Woofter, Jr. McGraw-Hill Book Co., New York. $2.50. 5⅞ x 9; xii + 247; 1933.

This is one of a series of monographs published under the direction of President Hoover's Research Committee on Social Trends. The author's object is to describe briefly the changes that have occurred in the racial composition of the United States and the social problems that have arisen as a consequence.

The various topics are, after a brief survey of immigration: the racial differences in movement and distribution, urban occupation, education, crime and assimilation. It is demonstrated that racial assimilation by intermarriage is exceedingly slow and with regard to the Negro legally impossible; so the author concludes that we must strive at least for cultural assimilation to eliminate interracial difficulties. This can be achieved only by real cooperation and mutual understanding between the members of the different ethnic groups.

It is evident that this book is not intended to be an original contribution but rather an outline of the findings of the

more recent investigators in the field. The author's extensive knowledge of the Negro leads him to devote most of this volume to the problems of this race, undue importance without doubt. An apparent oversight is the gross error on page 151 where cancer of the heart is mentioned. Probably cancer of the breast was intended.

HEALTH AND ENVIRONMENT.
By Edgar Sydenstricker. McGraw-Hill Book Co., New York. $2.50. 5¾ x 9; xii +217; 1933.

This is perhaps *the* outstanding monograph in the series of studies on Social Trends prepared for Mr. Hoover. Mr. Sydenstricker has not only put his material in such shape that it is readily comprehended by the general reader, but he has also done one of the most sagacious, scholarly, and thorough pieces of work in general biostatistics of this century. The special worker in this field will find it very valuable as a reference book. The author states in the preface that

> the factual material on the relation of environment to health is lopsided. On some phases great masses of data are available; on others the data either are wholly lacking or are pitifully few. Most of the material is indirect in its bearing upon the subject. is not sufficiently specific, and is discouragingly inconclusive. Yet a general view of it is worth while, and that is all this monograph pretends to offer.

The investigation has been limited to a review of the "more important evidence bearing upon environment and health, without, however, losing sight of the established laws of heredity." The data upon which the study is based are arranged in 54 tables. Numerous figures also exhibit the graphical arrangement of the data. The book is thoroughly documented and indexed.

LE TRAVAIL HUMAIN. *Revue Trimestrielle. 1re Année—No. 1.*.
Conservatoire National des Arts et Métiers, 292 Rue Saint-Martin, Paris. Subscription price: 100 francs (France and Colonies); 130 francs (foreign).

This new journal, edited by MM. J. -M. Lahy (*Directeur d'études à l'École des Hautes Études et à l'Institut de Psychologie de l'Université de Paris*) and H. Laugier (*Professeur à la Sorbonne et au Conservatoire National des Arts et Métiers, Paris*) expresses well its objectives in its potential sub-title, "Knowledge of man from the point of view of the judicious utilisation of his activities." It will be useful for all students of human biology, and thoughtful industrialists and other employers of labor as well. The position and repute of its editors will ensure the maintenance of high scientific standards. We wish this new venture all success and take great pleasure in recommending it to our readers.

A BIBLIOGRAPHY OF DIFFERENTIAL FERTILITY. *In English, French and German.*
Edited by Eldon Moore on behalf of Commission II of the International Union for the Scientific Investigation of Population Problems. International Union for the Scientific Investigation of Population Problems, Kensington Gore, London, S.W. 7. 2 shillings. 9¾ x 6; vi + 97; 1933.

This bibliography is a useful and welcome fruit of the work of the Commission on Differential Fertility, Fecundity, and Sterility of the International Union for the Scientific Investigation of Population Problems. It includes over 1,100 references—about 200 each in French and German and over 700 in English—arranged in chronological order. The English references range from Malthus' *Essay on the Principle of Population* in 1798 to 1931, the French—largely works published in Belgium—from 1838 to 1929, and the German from 1919 to 1930. Regular official publications are given in an appendix. The usefulness of the bibliography is enhanced by an index of authors.

THE WORLD SINCE 1914.
By Walter C. Langsam. The Macmillan Co., New York. $4.00. 5½ x 8½; xv + 723; 1933.

THE WORLD SINCE 1914. *Second Edition with Supplementary Chapter.*

By Walter C. Langsam. The Macmillan Co., New York. $4.00. 5¾ x 8½; xv + 742; 1933.

If you have forgotten or never knew the intricate pattern of events that led up to the World War, read this book. When you have read it, you will be less addicted to talking glibly about "war guilt" than perhaps you were before. Besides the excellent account of the origins and course of the war and of the "Peace to end peace" the book gives concise and complete political and economic histories of the various nations in the post-war period. It also contains a number of maps and illustrations, a voluminous bibliography of 33 pages and an index.

THE BUILDERS OF STONEHENGE. *Evergreen Essays No. 10.*

By Rendel Harris. W. Heffer and Sons, Cambridge, England. 3s. net. 6½ x 8¼; 73; 1932 (paper).

The author derives many English place names, especially near Stonehenge, from Egyptian originals and identifies various Cornish saints with Osiris masquerading in Christian vestments. On this basis he concludes that the Egyptians built Stonehenge for the joint worship of Rā and Osiris. It is true that both Stonehenge and the Egyptian temples are megalithic; yet if there had been such extensive Egyptian colonization in Britain as Mr. Harris postulates, sculptures or other artefacts of unmistakably Egyptian origin would surely have been discovered.

FLYNN OF THE INLAND. *Eighth Edition.*

By Ion L. Idriess. Angus and Robertson, Sydney. 6 shillings. 4⅞ x 7¼; xiv + 306; 1933.

In both style and context this book is about like every other account of the work of medical missionaries with this exception: there is less emphasis than usual on the beneficence of God and a great deal more on the wisdom of man in designing automobiles, airplanes, and radio sets. The scene is the interior of Australia, an almost barren country sparsely settled by white men, and the theme is the persistent effort of John Flynn to better the means of transportation and communication for himself and his co-workers. There is little of special interest to biologists or anthropologists.

RASSENKUNDE UND RASSENGESCHICHTE DER MENSCHHEIT. *Fünfte Lieferung (Bogen 37–46). Sechste Lieferung (Bogen 47–59 u. Titelbogen).*

By Egon F. von Eickstedt. Ferdinand Enke, Stuttgart. Lief. 5, 11 marks; Lief. 6, 13 marks. 7⅛ x 10⅛; Lief. 5, 160; Lief. 6, viii + 200 + folding map, 1933 (paper).

Previous numbers of this series have been noticed in the last two numbers of this REVIEW. By concluding the study of the negro populations of Africa, begun in *Lieferung 4*, and treating the races native to Oceania and the two Americas, the present numbers complete a series of excellent studies on the biology and history of the races of man. Number 6 provides author and subject indices for the whole series. A noteworthy addition to the anthropologist's library.

THE OLD STONE AGE. *A Study of Palaeolithic Times.*

By M. C. Burkitt. The Macmillan Co., New York. $2.50. 5¼ x 7½; xiv + 254; 1933.

An excellent introduction to the study of prehistory written especially for the student whose training has been along non-scientific lines. Special attention is given to the bases of the subject. The general reader will also find much to interest him, expecially those chapters dealing with prehistoric art and the motives underlying the art. The book is adequately illustrated and documented and contains a detailed index.

MENSCHLICHE AUSLESE UND RASSENHYGIENE (EUGENIK). *Vierte Auflage.*

By Fritz Lenz. J. F. Lehmanns Verlag,

München. 15.30 marks (Cloth); 13.50 marks (Paper). 6 x 8⅞; vii + 593; 1932. This is a reprint of the third revised and enlarged edition, published in 1931, of a well-known general treatise on eugenics. The book is the second of a series of two, the first of which, *Menschliche Erblichkeitslehre* by the late Dr. Erwin Baur, Eugen Fischer and the author of the present volume, has been translated into English.

ZOÖLOGY

FISHES: *Their Journeys and Migrations.*

By Louis Roule. Translated from the French by Conrad Elphinstone. W. W. Norton and Co., New York. $3.75. 5⅞ x 9¼; xii + 270; 1933.

An absorbing book, particularly for the general reader. At certain periods of the year all fishes, not only the migratory but the so-called sedentary fishes, change their abode. The journeys of the sedentary are within a restricted area. The migratory fishes often go great distances. In each case there is a definite cause—a requirement for warmer water or richer vegetation or water rich in dissolved oxygen, etc. In the case of the salmon, to which the author devotes several chapters, the comings and goings take place with great regularity. The young salmon, which remain until their second year in the water in which their early development takes place, go down the river by easy stages until the sea is reached, where they bury themselves in its depths. This urge to seek deeper water is held to be due to the fact that their pigment, which formerly was abundant and protected them against the strong sunlight which floods the river waters in the Springtime, has become diffuse. Their vitality is disturbed and they seek those regions better suited to healthy living. At the same time, the mature salmon, its body gorged with developing eggs or sperm, requires more vigorous respiration, hence the great migration up swiftly flowing streams to those waters containing the greatest amount of dissolved oxygen. Many other types of fish are discussed. In each case the author finds a definite requirement in the animal itself for a particular environment which causes it to migrate. The book is entertainly written. It contains many illustrations but unfortunately no index.

CORRELATION OF THE DISTRIBUTION OF THE PROTOZOA IN THE INTESTINE OF RATTUS NORVEGICUS WITH THE HYDROGEN ION CONCENTRATION OF THE INTESTINAL CONTENTS AND WALL. *University of California Publications in Zoölogy, Vol. 39, No. 8.*

By C. A. Kofoid, E. McNeil and A. Bonestell. University of California Press, Berkeley. 25 cents. 6¾ x 10¼; 12; 1933 (paper).

The authors report the following findings:

Trichomonas was found in the caecum of all rats examined. It was also present in the ileum in 40 per cent and in the colon in 80 per cent. There is a correlation between the decrease in numbers of *Trichomonas* in the upper ileum and the decrease in pH value of that region.

Hexamitus muris was present in the duodenum in 17 per cent, the jejunum in 7 per cent, the ileum in 57 per cent, and the caecum in 3 per cent of the rats examined. They are present at a pH range of 5.98–8.27.

Chilomastix bettencourti was present in the caeca of 60 per cent of the rats examined, at an average pH of 6.77.

Giardia was present in 60 per cent of all rats examined. It is always found in close proximity to the wall. Giardiasis is primarily an infection of the jejunum, occurring in greatest abundance at a pH range of 6.45–6.52.

Motile amoebae were present in 70 per cent of the rats examined, at an average pH of 6.7.

TEXTBOOK OF GENERAL ZOÖLOGY. *Second Edition, Rewritten and Reset.*

By Winterton C. Curtis, Mary J. Guthrie, with the collaboration of Katharine R. Jeffers. John Wiley and Sons, New York. $3.75 net. 6 x 9; xv + 588; 1933.

LABORATORY DIRECTIONS IN GENERAL ZOÖLOGY. *Second Edition, Revised.*

By Winterton C. Curtis, Mary J. Guthrie and Farris H. Woods. John Wiley and Sons, New York. $1.50 net. 5⅞ x 9; xxxii + 164; 1933.

Although some thirty pages shorter, there is much new material added to that of the first edition of this excellent elementary text. There has been such reorganization

of the subject matter that this second edition is really a new book, retaining, however, the good features of the earlier one. A glossary has been added and the index seems very complete.

The rearrangement of the text has necessitated a new edition of the Laboratory Directions which is designed to accompany it. Here the revisions are less extensive, consisting largely in changes in order.

The first edition of the text was noticed in Volume 3 of this REVIEW, the first edition of the Laboratory Directions, in Volume 1.

ANIMAL BIOLOGY. *First Edition.*

By Robert H. Wolcott. McGraw-Hill Book Co., New York. $3.50. 5⅞ x 9; xvii + 615; 1933.

This book is offered as another addition to the ever-increasing number of elementary zoölogical textbooks. The author has tried to minimize technicalities as much as possible and emphasizes that the volume is not a reference book. The question of animal structure is, on the whole, left to laboratory procedure. The book is divided into five general divisions as follows: Part I, Fundamental Principles; Part II, Protozoa; Part III, Metazoa in General; Part IV, Metazoan Phyla, and Part V, General Considerations. The author has commendably stressed dynamics and the book seems readable and authoritative. The value of this text is enhanced by the number of illustrations, many of which are original or have been redrawn.

HISTORY AND PRESENT STATUS OF THE BREEDING COLONIES OF THE WHITE PELICAN (PELECANUS ERYTHRORHYNCHOS) IN THE UNITED STATES. *Contribution of Wild Life Division Occasional Paper No. 1.*

By Ben H. Thompson. U. S. Department of the Interior, National Park Service, Berkeley. 6 x 9¼; vi + 85; no date (paper).

The breeding colonies of the white pelican are now only four. Once they extended over all the western half of North America. Lakes are the natural breeding grounds of this bird and their drainage for agricultural purposes is considered the principal cause of the reduction in numbers and density. The author does not consider the white pelican in immediate danger of being exterminated but feels that this will eventually be its fate unless means are taken to provide reservations where it can maintain and reproduce itself.

CONTRIBUTIONS TO THE BIOLOGY OF THE PHILIPPINE ARCHIPELAGO AND ADJACENT REGIONS. *United States National Museum Bulletin 100, Volume 12. The Fishes of the Families Banjosidae, Lethrinidae, Sparidae, Girellidae, Kyphosidae, Oplegnathidae, Gerridae, Mullidae, Emmelichthyidae, Sciaenidae, Sillaginidae, Arripidae, and Enoplosidae Collected by the United States Bureau of Fisheries Steamer "Albatross," Chiefly in Philippine Seas and Adjacent Waters.*

By Henry W. Fowler. U. S. Government Printing Office, Washington. 50 cents. 6 x 9¼; vi + 465; 1933 (paper).

This bulletin contains the fifth part of Fowler's studies of the fishes of the Albatross collections. It includes the main percoid series and embraces a great number of food or market fishes collected chiefly in the Philippine seas and adjacent waters. In the text are 32 black and white drawings. The work is extensively documented and there is a detailed index.

ÜBER DAS VERHALTEN VON STECHMÜCKEN BESONDERS VON ANOPHELES MACULIPENNIS BEI VERSCHIEDENEN TEMPERATUREN UND LUFTFEUCHTIGKEITEN.

By E. Martini and E. Teubner. Johann Ambrosius Barth, Leipzig. 6 marks. 6¼ x 9¼; 80; 1933 (paper).

This is the report of a comparative study of the influence of temperature and relative humidity on the behavior of several species of mosquitos under laboratory and field conditions. Differences in duration of life and in preference for combinations of temperature and humidity were found to exist in the species studied but these were not the most important controlling conditions in the open.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN, *Lieferung 416. Methoden der Tierhaltung und Tierzüchtung. Haltung und Züchtung von Vorratsschädlingen.*

By Friedrich Zacher. Urban und Schwarzenberg, Berlin. 12 marks. 7 x 10; 204; 1933 (paper).

This number of the *Abderhalden Handbuch* gives biological and ecological information concerning those insects, including moths, injurious to food and clothing, which may be used easily and cheaply for experimental purposes, together with directions for their cultivation in the laboratory. Numerous illustrations of the arthropods described and equipment for their culture, enhance the usefulness of the book.

A CHECK LIST OF NORTH AMERICAN AMPHIBIANS AND REPTILES. *Third Edition.*

By Leonhard Stejneger and Thomas Barbour. Harvard University Press, Cambridge. $4.00. 6 x 9¼; xiv + 185; 1933.

The second edition of this useful handbook appeared in 1923 and is now out of print. This third edition has been brought thoroughly up-to-date by the addition of much new material acquired in the last decade concerning the amphibians and reptiles of North America. As in previous editions the higher groups and genera are listed in systematic sequence and the species in alphabetical order.

CILIATES FROM BOS INDICUS LINN. III. *Epidinium Crawley, Epiplastron gen. nov., and Ophryoscolex Stein. University of California Publications in Zoölogy, Vol. 39, No. 1.*

By C. A. Kofoid and R. F. MacLennan. University of California Press, Berkeley. 45 cents. 6¾ x 10¼; 34; 1933 (paper).

REGULATORY DEVELOPMENT IN TRITURUS TOROSUS (RATHKE), *by Richard M. Eakin.* A ROTARY DISC FOR THE OBSERVATION OF OBJECTS IN PROFILE, *by J. Frank Daniel and A. B. Burch. University of California Publications in Zoölogy, Vol. 39, Nos. 9 and 10.*

University of California Press, Berkeley. 25 cents. 6¾ x 10¼; 13; 1933 (paper).

NEW SPECIES OF PROBOSCIDIELLA AND DEVESCOVINA FROM KALOTERMES OCCIDENTIS WALKER, A TERMITE OF LOWER CALIFORNIA. *University of California Publications in Zoölogy, Vol. 39, No. 3.*

By Walter W. Lewis. University of California Press, Berkeley. 35 cents. 6¾ x 10¼; 20; 1933 (paper).

A NEW BLIND ISOPOD, ASELLUS CALIFORNICUS, AND A REVISION OF THE SUBTERRANEAN ASELLIDS. *University of California Publications in Zoölogy, Vol. 39, No. 4.*

By Milton A. Miller. University of California Press, Berkeley. 25 cents. 6¾ x 10¼; 13; 1933 (paper).

STUDIES ON THE STRUCTURE AND LIFE-HISTORY OF OSTIOLUM OXYORCHIS (INGLES) FROM THE CALIFORNIA RED-LEGGED FROG RANA AURORA DRAYTONI AND STUDIES ON THE STRUCTURE AND LIFE-HISTORY OF ZEUGORCHIS SYNTOMENTERA SUMWALT, A TREMATODE FROM THE SNAKE THAMNOPHIS ORDINOIDES FROM CALIFORNIA. *University of California Publications in Zoölogy, Vol. 39, Nos. 6 and 7.*

By Lloyd G. Ingles. University of California Press, Berkeley. 50 cents. 6¾ x 10¼; 44; 1933 (paper).

A NEW SPECIES OF ECHINOID FROM TAMAULIPAS, MEXICO, *by Merle C. Israelsky.* A NEW GRYPHAEOID OYSTER FROM THE EOCENE OF CALIFORNIA, *by Leo G. Hertlein. Transactions of the San Diego Society of Natural History, Vol. VII, No. 22.*

San Diego Society of Natural History, San Diego. 6¾ x 10¼; 9; 1933 (paper).

ANNUAL REPORT OF THE DIRECTOR OF THE MUSEUM OF COMPARATIVE ZOÖLOGY AT HARVARD COLLEGE TO THE PRESIDENT OF HARVARD COLLEGE FOR 1932–1933.

By Thomas Barbour. Museum of Comparative Zoölogy, Cambridge. 6 x 9⅛; 69; 1933 (paper).

BOTANY

PLANTS USEFUL TO MAN.

By Wilfred W. Robbins and Francis Ramaley. P. Blakiston's Son and Co., Philadelphia. $3.00. 5½ x 8¼; vii + 428; 1933.

This textbook is intended for students who want to be introduced to usable information about plants with the fewest preliminaries possible and it ought to be useful as a textbook in one-term botany courses in agricultural colleges. About half of the

material has been taken from Robbins' *Botany of Crop Plants*. It is well illustrated and indexed.

How Plants Get Their Names.

By L. H. Bailey. The Macmillan Co., New York. $2.25. 5¼ x 7¾; vi + 209; 1933.

The title of this book and the reputation of its author are almost sufficient, by themselves, to characterize this introduction to systematics intended for horticulturists and nature lovers in general. Bailey makes a story out of the nomenclatorial history of *Solanum PseudoCapsicum* and *Capsicum frutescens* in order to lead up to the work of the great systematists, and the manner and the spirit in which they worked. It is a cleverly written defense of taxonomy as a living science and a necessary one, and its objective is to remove some of the antagonism to scientific nomenclature, and its occasional changes, that is all too prevalent. The book belongs on reading lists in college botany courses. More than 80 pages are devoted to lists of generic and specific names, with the accents marked to aid in pronunciation. The specific names are also translated.

Three Keys to Wild Flowering Plants *of Connecticut, Southeastern New York, New Jersey and Eastern Pennsylvania.*

By Mary F. Barrett, 64 Park Ave., Bloomfield, N. J. 50 cents. 6 x 9; 46; 1933 (paper).

Ecological characters easily recognized by untrained persons have been used wherever possible for purposes of classification; for instance, plants flowering in spring or in autumn fall in different sections, and stem and leaf characters, rather than floral ones, are used to divide up aquatic plants. This manual is meant to serve as a guide for beginners to the larger standard floras; accordingly detailed descriptions of the species are omitted. There is a very explicit and elementary glossary supplemented by illustrations, and a good index of scientific and popular names. It ought to be very useful for "teachers, students, Scout leaders and other interested persons who want simple aids to identification."

The Study of Cacti.

By Vera Higgins. Blanford Press, London. 7s. 6d. net. 5¼ x 8⅜; 164; 1933.

This book has been prepared especially for the amateur cultivator of cacti. Mrs. Higgins has made a very useful book. She assumes that those interested wish to acquire considerable general information concerning these remarkable plants as well as to have a guide for their cultivation. The sections on nomenclature and descriptions of genera are well planned. The volume is illustrated and contains a bibliography, a chart of Britton and Rose's classification, and an index.

The Pneumatic System of Plants, Especially Trees. *Carnegie Institution of Washington Publication No. 441.*

By D. T. MacDougal and Earl B. Working. Carnegie Institution of Washington. 75 cents. 6⅞ x 10; 87; 1933 (paper).

The gases found in the wood of tree trunks differ little in pressure from the atmosphere but their oxygen and carbon dioxide content is subject to very wide variation. Mac Dougal and Working conclude that their data on the movement of gases through wood, as well as the results of their chemical analyses, indicate that there are facilities for gaseous streaming not yet demonstrated by anatomical studies and that there is no satisfactory explanation for the high CO_2 tensions they find.

An Introduction to Laboratory Technique in Bacteriology. *Revised Edition.*

By Max Levine. The Macmillan Co., New York. $1.75. 5 x 7⅞; xiii + 289; 1933.

This is the second edition of an excellent laboratory manual designed, in its 114 exercises, to give the student an introduction to the important bacteriological procedures. There is a short dichotomous key for the identification of some commonly encountered bacteria, an appendix with

formulas for media, stains, and reagents, and a glossary which supplements drawings of growth habits.

NOTES ON THE FORAMINIFERA OF THE TYPE MERCED AT SEVEN MILE BEACH, SAN MATEO COUNTY, CALIFORNIA. *Transactions of the San Diego Society of Natural History, Vol. VII, No. 21.*

By Roscoe E. and Katherine C. Stewart. San Diego Society of Natural History, San Diego. 6¾ x 10¼; 13; 1933 (paper).

MORPHOLOGY

A CORRELATION OF THE SILVERLINE AND NEUROMOTOR SYSTEMS OF PARAMECIUM. *University of California Publications in Zoology, Vol. 39, No. 2.*

By Everett E. Lund. University of California Press, Berkeley. 65 cents. 6¾ x 10¼; 42; 1933 (paper).

This study was undertaken to harmonize the conflicting views concerning the fibrillar system of *Paramecium*. A review of the literature is given and the silverline system as demonstrated particularly by Klein's technique, and the neuromotor system as demonstrated by the use of iron-haematoxylin and Mallory's stain are described in detail and compared. The conclusion is reached that

> Because the silverline system confuses parts of three very different structures, the pellicle, trichocysts, and neuromotor complex; because it does not include more than a small part of the entire coördinating system; and because the name "silverline" is descriptive of the technique used to demonstrate the "system" rather than the structures themselves, the term "neuromotor system" is preferable when referring collectively to the conductile elements of ciliate Protozoa.

The study includes excellent line drawings and plates and a literature list.

LYMPHATICS, LYMPH AND TISSUE FLUID.

By Cecil K. Drinker and Madeleine E. Field. The Williams & Wilkins Co., Baltimore. $3.00. 5⅜ x 8; xv + 254; 1933.

An excellent survey of what is known of the physiology of the mammalian lymphatic system. The authors, both experimentalists in this field, have drawn their data from anatomy, physiology, pathology and immunology, clearly defining the problems involved and presenting their own well formulated views. As a source book the volume will be most useful in experimental laboratories. Experimental data are arranged in tables and figures. There is a lengthy bibliography, an author index, and an excellently arranged subject index.

DIE EINTEILUNG DES NERVENSYSTEMS NACH SEINEN LEISTUNGEN.

By L. R. Müller. Georg Thieme, Leipzig. 6.80 marks. 6¾ x 9¾; 74; 1933 (paper).

A short study of the components of the nervous system classified according to performance into (1) the *Systema nervorum pro mundo*, that is the part of the nervous system dealing with the perception of and reactions to the external world (the sensory and motor system), (2) the *Systema nervorum myostaticum*, controlling the tonus of the muscles and tendons, and (3) the *Systema nervorum vitale* which includes the sympathetic nervous system. In the last section an attempt is made to localize centers governing psychical acts. The author concludes with an expression of wonder and awe for the god who invented and created this complicated system. There are nineteen illustrations, mostly diagrammatic drawings.

PHYSIOLOGY AND PATHOLOGY

STUDIES ON THE PHYSIOLOGY OF THE EYE. *Still Reaction, Sleep, Dreams, Hibernation, Repression, Hypnosis, Narcosis, Coma, and Allied Conditions.*

By J. Grandson Byrne. H. K. Lewis and Co., London. 40 shillings net. 6 x 9½; xii + 428; 1933.

This book is a report on twenty years of research on the eye. The cat served as the chief source of material. However, a variety of animals, as well as man, came under consideration at times. The author

has thoroughly gone over the cat with a precise technique of nerve section and tissue traumatization augmented by use of specific drugs such as adrenalin and ergotoxin. His chief contribution lies in his record of acute and detailed observations on the eye responses in both normal and experimental situations. An excellent index to the book's content is given in the author's conclusion:

> The functional relations established in the present studies between the projicient (distance) receptor organs and the mechanisms which mediate the visible, physical reactions (objective components) in approach, avoidance or standstill of the intermediate level, and those which mediate reactions in 'approach,' 'avoidance' and 'standstill' (still reaction) of the higher (psychic) level have their manifest implications from the standpoint of a rational psychology, since every reaction of the individual can be identified as a descendant of one or other of the three basic or pattern forms of reaction in actual approach, avoidance, or standstill of the intermediate and of the lower level; or as one or other of the developmental derivatives (specializations) of the basic reactions as mediated at the higher (psychic) level, viz. reactions in 'approach,' 'avoidance,' or 'standstill' (still reaction). It seems, therefore, that these relations furnish the means for a better understanding of the different factors concerned in the physiology of vision. They also, in my opinion, furnish the means for a better understanding of the physiological processes that underlie, or are associated with, all mental activity, and consequently may furnish in addition the solid foundation for a therapy—preventive and restorative, based upon psychoanalysis—which shall not be too mystifying or metaphysical, but scientific and practical, and above all available for extensive use by the medical practitioner.

THE RISE OF PREVENTIVE MEDICINE. *University of London Heath Clark Lectures, 1931. By Sir George Newman. Oxford University Press, New York.* $3.00. 5½ x 8½; 270; 1932.

A pleasantly written history of medicine compiled from standard sources. The chief criticism that might be made of it is that it seems unnecessarily discursive and general, instead of being precisely aimed at its announced subject, *preventive* medicine. Also the author seems a bit more optimistic than some would be in his evaluation of the present effectiveness and power of public health measures. But this is perhaps to be expected, and is certainly pardonable, in a man of Sir George's official position. The book is well illustrated and indexed.

CANCER AND OTHER CHRONIC DISEASES IN MASSACHUSETTS.

By George H. Bigelow and Herbert L. Lombard. Houghton Mifflin Co., Boston. $4.00. 5⅝ x 8½; xx + 355; 1933.

In Massachusetts cancer is now considered a public health problem and there have been established 12 state clinics and a hospital for the diagnosis and treatment of this disease. The authors here describe the advantages derived from these clinics and seek to demonstrate the necessity of establishing more of them, not only for cancer but also for other chronic diseases. They desire to develop a program beneficial for the people and still not economically harmful to physicians.

Almost half of this book is composed of statistical tables relative to the population of Massachusetts; incidence and estimated cost of chronic diseases, incidence of cancer in general, and cancer of particular organs; with respect to age, sex, and in some cases, nativity of patients. The data given will be found useful though they do not present facts much different from those observed elsewhere.

LIFE IN THE MAKING.

By Alan F. Guttmacher. The Viking Press, New York. $2.75. 5¾ x 8¼; xii + 297; 1933.

The progress of knowledge regarding human reproduction is here outlined in an interesting manner, and simply enough to be understood by the layman. The theories of the past and those of the present are ably summarized and intermingled with well chosen anecdotes as well as with recorded scientific observations. The subject matter ranges from description of the elements of reproduction to sexual behavior, sex differences and sex determination to sterility and fertility. One section is devoted especially to twins. It seems odd that here the author, who is by profession an

obstetrician, should lay such heavy stress upon physical resemblance as the criterion for distinguishing monozygotic from dyzygotic twins, and so little upon the condition of the foetal membranes.

The author's concise and accurate treatment of the subject should serve to give to the layman a better knowledge of human reproduction than he now has and dispel from his mind age-old misconceptions. This end would perhaps be even better achieved if in all places the proven facts had been more clearly separated from mere hypotheses.

COMPARATIVE PHYSIOLOGY OF THE GASTRIC HUNGER MECHANISM.

By Thomas L. Patterson. New York Academy of Sciences, New York. $2.00. 6¼ x 9½; 218; 1933 (paper).

This paper was awarded the A. Cressy Morrison Prize in Experimental Biology for 1931 by the New York Academy of Medicine. In it Professor Patterson has reviewed critically and summarized all that is known of the gastric hunger mechanism. The following headings, taken from the table of contents, will indicate the scope of the work: (1) The biological significance of hunger (including hunger in the protozoa, metazoa, plants, and higher animals). (2) Comparative physiology of gastric motility in vertebrate and invertebrate animals. (3) Types of animals studied (this includes the Arthropoda, Mollusca and Chordata). (4) The gastro-neuro-muscular mechanism (including the nature of the gastric response, and the influence of stimulation of the sciatic, vagus, and splanchnic on the tonus and motility of the gastric mechanism). Taken together with its bibliography of some 250 references, this is a contribution of the first order. In concluding the author suggests that "a hypothetical tonus center may exist in the brain" and that the "dorsal vagus nucleus may act as a tonal center for the hollow visceral organs (stomach, intestines, etc.) and be somewhat analogous in its action to that of the vaso-motor center in the maintenance of tone of blood vessels."

DER STOFFAUSTAUSCH ZWISCHEN MUTTER UND FRUCHT DURCH DIE PLACENTA.

By H. Schlossmann. Julius Springer, Berlin; J. F. Bergmann, Munich. 6.60 marks. 6¾ x 10¼; ii + 73; 1933 (paper).

The author reviews the observations which have been made on the metabolic exchanges between mother and fetus through the placenta and evaluates critically the experimental methods employed and the theories advanced by various workers. His conclusion agrees with that of Slemons in this country, namely that transmission from mother to fetus, or *vice versa*, is by simple physical processes alone, such as diffusion, and that vitalistic elements play a small part, if any. The contents of the book include: morphology of the placenta, paths of exchange between mother and fetus, metabolism of the placenta itself, responses of the vessels and umbilical cord, experimental methods, transmission through the placenta of carbohydrates, proteins, lipoids, fats, hormones, vitamins, salts and other substances normally found in the blood of mother and fetus, and foreign substances. This is an important monograph, well-written and concise. The bibliography covers eight pages.

THE HISTORY AND EPIDEMIOLOGY OF SYPHILIS. *The Gehrmann Lectures, University of Illinois.*

By William A. Pusey. Charles C Thomas, Springfield, Ill. $2.00. 5¾ x 8¾; xii + 113; 1933.

In these lectures the distinguished author traces the beginning of syphilis in Europe, the development of our knowledge of it, and its epidemiology. Unlike Sudhoff, he does not credit the existence of the disease in Europe in pre-Columbian times. He quotes Oviedo, Las Casas and Dias de Isla, all of whom testify that syphilis was brought back to Spain from the West Indies by the members of Columbus' expedition and thence introduced into Italy. As to "*gros mal*," references to which Sudhoff found in pre-Columbian edicts, and which he identified with syphilis, Pusey concludes that the term was used to denote,

not syphilis, but epilepsy. The book contains interesting illustrations from early books on syphilis, portraits of eminent syphilologists from Paracelsus to Ehrlich, and an index.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 412. Quantitative Stoffwechseluntersuchungen.* Containing following articles: *Bestimmung des respiratorischen Quotienten von überlebenden Geweben*, by Frank Dickens and František Šimer; *Der Helm-Respirationsapparat in seinen verschiedenen Formen*, by Francis G. Benedict; *Messung des Gasstoffwechsels mit der Apparatur nach Simonson*, by Ernst Simonson and Hermann Hebestreit; *Ein Apparat zur Analyse von Gasen aus Respirationskammern für Menschen und Tiere*, by Thorne M. Carpenter.

Urban und Schwarzenberg, Berlin. 8.60 marks. 7 x 10; 184; 1933 (paper).

Dickens and Šimer discuss certain refinements of the Warburg technique useful in determining the respiratory quotient in slices of living tissue.

Benedict contributes a long section on the use of the helmet used in the Nutrition Laboratory of the Carnegie Institution in the study of respiration in man. Carpenter describes the construction, calibration, and use of the gas analysis apparatus developed in the same laboratory.

Simonson has an apparatus for diverting an aliquot portion of the respired air into small rubber balloons, whose contents can be analysed later, the main stream of respired air passing through a gas meter set on the ground wherever convenient. He claims that the subject is much less encumbered by his apparatus than by the Douglas-Haldane one. He gives tables to aid in computation.

STUDIES ON THE NUTRITIVE VALUE OF MILK. *II. The Effect of Pasteurization on Some of the Nutritive Properties of Milk. Bulletin 518.*

By W. E. Krauss, J. H. Erb, and R. G. Washburn. Ohio Agricultural Experiment Station, Wooster. 6 x 9; 33; 1933 (paper).

The authors have conducted an experiment on 32 pairs of rats to investigate certain inferior nutritive qualities attributed to milk when pasteurized.

They find that in rats fed milk exclusively, nutritional anemia develops at about the same rate in those given raw as in those given pasteurized milk. The anemia is apparently due to the lack of copper and iron in the milk itself and disappears equally in both groups of rats when these elements are added to their diet. The ash content of the femurs and of the entire bodies revealed the same percentages of calcium and phosphorus in the two groups. The authors also observe that pasteurization destroys about 25 per cent of vitamin B, but does not alter vitamins A, G, and D.

Their results appear conclusive but do not agree in many points with those of other observers, so the controversy on the effect of pasteurization may be expected to go merrily on.

THE HUMAN BODY AND ITS FUNCTIONS. *An Elementary Text-book of Physiology.*

By C. H. Best and N. B. Taylor. Henry Holt and Co., New York. $3.00 (Student ed.); $3.75 (Trade ed.). 5½ x 8⅜; xiii + 417; 1933.

This introduction to human physiology is written in more colloquial language than most textbooks intended for college students, and in general it has been planned for students approaching the subject with a minimum of scientific preparation. Most of the topics of human physiology of current interest are surveyed and there is a commendable tendency to compare the functions of the lower organisms with those of man. There are numerous illustrations, most of them diagrammatic drawings, some of them colored, a pronouncing glossary, and a good index. Because of the freedom from technical language and the care used to explain the basic physics and chemistry of the various processes it should be useful to laymen who happen to have any curiosity about this field.

STUDIES IN THE HISTORY OF OPHTHALMOLOGY IN ENGLAND PRIOR TO THE YEAR 1800.

By R. Rutson James. The Macmillan Co., New York; Cambridge University Press, Cambridge. $4.00 (U.S.A.); 15 shillings net (England). 6⅛ x 9⅛; x + 255; 1933.

The author draws on his own previously published papers for much of the material in this book. The earlier practices and development of ophthalmology are presented in most readable form. As contrasted with modern ideas the practices take on an air of quackery. However, the progress of sound knowledge is traced from the thirteenth century onward. The eighteenth century claims at least half the book. In spite of the record of substantial progress during that time, particularly in the treatment of cataract, the most picturesque reading concerns the performances of quacks who cleverly if not always skilfully treated the royalty of that period.

FOOD, NUTRITION AND HEALTH. *Third Edition, Rewritten.*

By E. V. McCollum and J. Ernestine Becker, East End Post Station, Baltimore. $1.50. 5 x 7⅜; v + 146; 1933.

A third edition of a book which has been previously mentioned in these pages. The present volume has been rewritten to include all the important findings of recent studies. Written in simple language, those without technical training and particularly the housewife will find it most useful in the proper planning of meals for the maintenance of health in the family as well as an economic expenditure of money. Many theories and claims of faddists and food promoters are exploded. The work concludes with a section on the distribution of vitamins in food stuffs. There is an index.

A TEXTBOOK OF PHYSIOLOGY *for Medical Students and Physicians. Twelfth Edition, Thoroughly Revised.*

By William H. Howell. W. B. Saunders Co., Philadelphia. $7.00 net. 5¾ x 9⅛; 1132; 1933.

The twelfth edition of this well-known textbook brings each chapter into line with modern research. The most extensive additions are on the chemistry of muscle contraction, the hormones, and particularly the vitamins.

BIOCHEMISTRY

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 415. Fermentforschung. Methodik des Nachweises von Abwehrfermentwirkungen.*

By Emil Abderhalden. Urban und Schwarzenberg, Berlin. 3.50 marks. 7 x 10; 70; 1933 (paper).

This volume describes in detail the author's work on what has been called the Abderhalden reaction. This was originally described as a means of diagnosing pregnancy. Its bases consisted in the idea that the maternal organism elaborates a ferment to protect itself against the foreign protein absorbed through the chorionic villi from the embryo. This protective ferment by enzymatic action destroyed the foreign embryonic material.

Although Abderhalden's original test has proven of little practical value and in general was long ago discredited as a method of diagnosing pregnancy, the general principle of an organism being able to elaborate ferments as a protection against foreign proteins has found considerable credence. He has therefore elaborated the original work and extended it to other conditions (e.g. dementia praecox, *eclampsia gravidorum*, etc.) which presumably are due to invasion by foreign proteins.

CHEMICAL INVESTIGATIONS OF THE TOBACCO PLANT. *Carnegie Institution of Washington Publication No. 445.*

By Hubert B. Vickery, George W. Pucher, Alfred J. Wakeman and Charles S. Leavenworth. With Technical Assistance of Laurence S. Nolan. Carnegie Institution of Washington. $1.00. 6⅞ x 10; 77; 1933 (paper).

A biochemical study of mature tobacco leaves detached from the plant and hung in a curing shed was divided into two

parts. In one set of leaves the chemical changes that occur during the usual curing process, and which involve chemical transformations of the greater proportion of the materials in the leaf, were considered. In another set, the leaf bases were allowed to dip into water in order to follow the metabolism of turgid leaves in dim light. The detailed results deserve the attention of specialists. Perhaps the most interesting feature, as far as non-specialists are concerned, is that nicotine appears to be an intermediary product of nitrogen metabolism, and decreases in amount during the curing process in some unknown way.

THE BIOCHEMISTRY OF MEDICINE.

By A. T. Cameron and C. R. Gilmour. William Wood and Co., Baltimore. $7.25. 5½ x 8¾; x + 506; 1933.

The principal clinical applications of biochemistry are described in brief terms and the significance of the chemical tests critically evaluated. It is this, together with the numerous references, that renders this volume a valuable handbook on the subject. The authors consider only those diseases of which the manifestations and symptoms can be explained on a biochemical basis. For purposes of clear exposition it is in effect assumed that the whole question is one of chemistry, and the disease or condition is treated almost as an abstract chemical problem. Especially concise and clear is the outline of human biochemistry which precedes the descriptive part. A useful and stimulating book.

THE PHYSICO-CHEMICAL THEORY OF THE PROCESS OF THE INTERNAL DEFENCE OF ANIMALS. (*Immunity, Tolerance, Specific Chemiotherapy and the Related Phenomena of Hypersensitiveness and Idiosynchrasis*) (*A Preliminary Report*).

By David S. Anand, Infectious Diseases Hospital, Lahore, India. Rs. 3. 5¼ x 8½; xii + 92; no date (paper).

Anand draws an analogy between the parenteral digestion of proteins in the alimentary canal and the internal hydrolysis of foreign proteins or antigens in the circulatory system. According to his theory the hydrolysis of antigens is brought about by catalysts derived from the reticulo-endothelial cells. In the process of hydrolysis antigens are converted by successive stages into antibodies. The pamphlet is not well written and is very poorly printed. In the review copy a number of pages are duplicated and several are missing.

ZUM MECHANISMUS DER ENZYMWIRKUNG UNTER BESONDERER BERÜCKSICHTIGUNG DER KRYOLYSE. *Sammlung chemischer und chemisch-technischer Vorträge, Neue Folge Heft 18.*

By F. F. Nord. Ferdinand Enke, Stuttgart. 4.30 marks. 6½ x 10; 51; 1933 (paper).

An account of the experiments conducted in the Physiological Institute of the Veterinary School in Berlin, on the mechanism of enzyme action, with particular reference to the effects of freezing. Freezing solutions and emulsions and then making measurements of surface tension, viscosity, electric conduction, cataphoretic migration velocity, gas absorption and drop count, showed that in freezing the lyophile colloids underwent an irreversible change. In similar measurements the direction of change, with exception of viscosity, was similar for all colloids. The bibliography contains six titles.

CHEMISTRY AND PHYSICS FOR BOTANY AND BIOLOGY STUDENTS. *Second Edition.*

By E. R. Spratt. University Tutorial Press, London. 3s. 6d. 4¾ x 7⅞; vii + 284; 1933.

A second edition, thoroughly revised, of a book which is highly useful in teaching the elementary biochemistry of plants and animals to students of elementary botany and of biology in general. Many of the sections have been extended. New chapters on magnetism and electricity, electrolytic dissociation, the atom and valency, light, etc. have been added. Each section concludes with a group of experimental problems for laboratory work. The work is abundantly illustrated and is indexed.

BIOCHEMICAL AND ALLIED RESEARCH IN INDIA IN 1932.

Society of Biological Chemists, Indian Institute of Science, Bangalore. Subscription Rs. 2 per year. 5½ x 8½; 86; 1933 (paper).

SEX

TESTICULAR GRAFTING FROM APE TO MAN: *Operative Technique, Physiological Manifestations, Histological Evolutions, Statistics.*

By Serge Voronoff and George Alexandrescu. Translated by Theodore C. Merrill. Williams and Norgate, London. 5 shillings net. 6¼ x 8⅝; viii + 125; 1933.

THE CONQUEST OF LIFE.

By Serge Voronoff. Translated by G. Gibier Rambaud. Williams and Norgate, London. 7s. 6d. net. 5½ x 8½; 201; 1933.

These books are representative of two aspects of the work of Voronoff and his coworkers. The first book, addressed to the medical profession, deals with technical details of glandular grafting. It is primarily an attempt to establish the scientific soundness of Voronoff's methods and results. The second book, addressed to the general public, is an attempt to popularize "rejuvenation."

Testicular Grafting is divided into four parts. The first deals with the operative technique which has led to successful transplanting of thin sections of simian testicular tissue upon the tunica vaginalis of the human testicle. In the second part the physiological effects on the grafted individual are described. Even the most sympathetic critical reader will probably fail to be impressed by vague subjective reports that the grafted individual "feels better." The third portion of the book contains a discussion of changes which the grafted tissue undergoes. A number of histological drawings show the initial plasma and cellular infiltration into the graft, the reorganization of the epithelial elements of the seminiferous tubules into insular epithelial structures, and the final disappearance of the epithelial tissue and complete fibrosis of the graft after a period of six to eight years. This section is by far the most important in the book. The last portion of the book is designated "Statistical Summary." Here a short table gives the indications for grafting and the results obtained on 475 human cases operated on by Drs. Serge and Georges Voronoff. The "results" consist of the unqualified classification of the cases by the operators into "successes" and "failures."

The *Conquest of Life* may do much to discredit Voronoff in the minds of his scientific contemporaries.

MAN INTO WOMAN. *An Authentic Record of a Change of Sex. The True Story of the Miraculous Transformation of the Danish Painter Einar Wegener (Andreas Sparre).*

Edited by Niels Hoyer. Translated from the German by H. J. Stenning. Introduction by Norman Haire. E. P. Dutton and Co., New York. $3.50. 5¾ x 8½; 288; 1933.

This book contains an account of an attempted surgical transformation of a man into a woman. Andreas Sparre, a well-known Danish painter, was obsessed with the idea that his destiny was to become a woman, not only in spirit but also in fact. After a long search he found a German physician, Professor Warnekros, an endocrinologist and surgeon of Dresden (his name is mentioned on the blurb but not in the text) who encouraged him in his effort and made arrangements for the surgical transformation. Sparre, outwardly, was definitely male but hormonal blood tests revealed the presence of female sex hormones and later rudimentary ovaries damaged by previous X-ray treatment were found. In several operations the male organs were completely removed and ovarian tissue from a healthy young girl was implanted.

At this stage Sparre had his name changed to Lili Elbe and legally became a woman, had his marriage annulled and became engaged to a French painter. This extraordinary story is finally concluded by the death of Lili Elbe, following an attempt to build-in the necessary apparatus for intercourse and child bearing.

After the first fifteen pages in which the general outlines of this account are given, the book becomes dull reading. The history of the biological as well as the psycho-

logical details, as told chiefly by Sparre to a friend, is too incomplete to make the book of any particular interest beyond that of an extraordinary medical anecdote.

SANE REASONS FOR BIRTH CONTROL. *Partington's Plain Talks No. 1.*
By T. Bowen Partington. Health and Strength, London. 6d. net. 4¼ x 7⅜; 31; no date (paper).

A LEARNED JUDGE'S VIEWS ON BIRTH CONTROL. *Mr. Justice McCardie Speaks Out. Partington's Plain Talks No. 2.*
By T. Bowen Partington. Health and Strength, London. 6d. net. 4¼ x 7⅜; 29; no date (paper).

THE SECRET VICE OF YOUTH *and Its Effect in Recent Years. Partington's Plain Talks No. 3.*
By T. Bowen Partington. Health and Strength, London. 6d. net. 4¼ x 7⅜; 30; no date (paper).

Pamphlets 1 and 2 contain the usual sentimental appeal of the necessity of birth control for the poor people. The sincerity of T. B. Partington, F.I.L., F.R.E.S., is so apparent that one would almost suspect him of advocating a law making birth control obligatory. Incidentally, for a small fee, the author is prepared to give advice to those troubled with sex problems.

The pamphlet on masturbation is very feeble even when compared with the above. The author would certainly benefit by glancing through the works of any of the religious writers on the subject.

SEX AND REJUVENATION.
By G. E. O. Knight. New World Publishing Co., London. 1 shilling. 5 x 7½; 18; no date (paper).

SEX FROM THE STANDPOINT OF YOUTH.
By R. H. Innes. New World Publishing Co., London. 1 shilling. 5 x 7½; 16; no date (paper).

What would be the fate of our civilization if we had not had such men as these authors to guide us?

There will be no more immorality, says Mr. Innes, when free love and nudism become universal. This conclusion is arrived at with such gems of thought as the following: "Free love is sexual intercourse sublimated by love."

On the other hand Mr. Knight has at heart the physical ills due to endocrine disfunction. These are many he says, without enumerating, and sex rejuvenation will relieve them. Of course the evil minded might think the author mercenary only because he mentions a certain name and address where sex rejuvenation is to be achieved.

BIOMETRY

USE OF THE EXPONENTIAL YIELD CURVE IN FERTILIZER EXPERIMENTS. *U. S. Department of Agriculture Technical Bulletin No. 348.*
By W. J. Spillman. U. S. Government Printing Office, Washington. 10 cents. 5⅞ x 9; 66; 1933 (paper).

Although the law of diminishing returns has long been a subject of discussion among economists, the earlier members of the profession did little to investigate the actual functional form of the relation between amount of a given factor in production and amount of product. It was left for agronomists and agricultural economists to determine this relation in their experiments on the effect of different amounts of a growth factor supplied in fertilizer on the crop produced. In 1911 Mitscherlich found that an exponential curve of the form $Y = M - AR^x$ describes the relation, while in 1927 Niklas and Miller concluded that it could be better represented by a second order parabola. The author of this bulletin prefers the exponential form, since it is much more successful in extrapolation than the parabola. Formulas are derived for determining the amount of available plant food in the soil, the optimum fertilizer formula to use, and the optimum quantity of fertilizer to apply for greatest profit per acre. Methods and auxiliary tables for the fitting of exponential curves are given. There is a bibliography of 11 titles.

THE NOMOGRAM. *The Theory and Practical Construction of Computation Charts.*
By H. J. Allcock and J. Reginald Jones.

Isaac Pitman and Sons, New York. $3.00. 5½ x 8⅜; viii + 209; 1932.

One of the most useful methods for the rapid solution of complex numerical formulae is the nomogram or computation chart. However, this device is still less extensively used than its advantages warrant. The authors have aimed in this excellent book to combine a discussion of the general theory with practical directions for the construction and use of all classes of computation charts having scientific or industrial applications. In every case, stress has been laid on the details of construction, and examples are given illustrating the various methods employed.

MANUAL OF JOINT CAUSES OF DEATH. *Showing Assignment to the Preferred Title of the International List of Causes of Death When Two Causes are Simultaneously Reported. Department of Commerce, Bureau of the Census. Third Edition.*

Prepared under the supervision of T. F. Murphy. U. S. Government Printing Office, Washington. $1.00. 5¾ x 9; iii + 255; 1933.

PSYCHOLOGY AND BEHAVIOR

THE INTELLIGENCE OF SCOTTISH CHILDREN. *A National Survey of an Age-Group. Publications of the Scottish Council for Research in Education V.*

The Scottish Council for Research in Education. Mental Survey Committee. University of London Press, London. 5 shillings net. 5¾ x 8⅜; x + 160; 1933.

A survey representing a complete cross-section of the Scottish community. The volume will be highly useful to those planning similar investigations in other countries. Roughly 87,000 children born in 1921 were tested, the survey taking place in June 1932. Scholarship tests devised by the committee in charge of the work together with the Terman revision of the Binet scale were made on a thousand children for purposes of comparison with the group tests. The results show no certain evidence of any significant difference in average intelligence between the boys and girls born in Scotland in 1921. There seems fairly definite evidence that the intelligence quotients of the boys are more widely scattered than the intelligence quotients of the girls.

The data indicate that the scatter of intelligence is considerably greater than previous surveys have shown. If I.Q. 90 to I.Q. 110 is taken to represent the range of what is termed the average child, somewhat fewer than half of the school population are in this sense average, and not somewhat more than half, as has hitherto been held. The groups which range themselves on either side of the average and are usually termed superior and retarded respectively are greater than has hitherto been believed.

The report is complete in every detail. The data on individual and group tests are exhibited in tables and graphs in the text. In a group of appendices will be found instructions for giving the tests, forms of tests, etc. There is an index.

A HANDBOOK OF CHILD PSYCHOLOGY. *Second Edition, Revised.*

By John E. Anderson, Phyllis Blanchard, William E. Blatz, Charlotte Bühler, Barbara S. Burks, Leonard Carmichael, Arnold Gesell, Florence L. Goodenough, Leta S. Hollingworth, Harold E. Jones, Mary C. Jones, Vernon Jones, Heinrich Klüver, Kurt Lewin, Dorothea McCarthy, Margaret Mead, Joseph Peterson, Jean Piaget, Rudolf Pintner, Karl C. Pratt, Mary M. Shirley, Lewis M. Terman, Lee E. Travis, Beth L. Wellman. Edited by Carl Murchison. Clark University Press, Worcester, Mass. $5.00. 6¼ x 9; xii + 956; 1933.

The editor states in the preface that the revision of this volume "is due chiefly to the great expansion of the field during the past three years and partly to the improved insight of the Editor." While the change in editorial insight may not be apparent, the great growth of the field is plainly evident. Of the twenty-two papers included in the first edition, four, *Children's Philosophies*, *Eidetic Imagery*, *The Feebleminded Child*, and *The Primitive Child* remain substantially unchanged. Seven papers, *The Methods of Child Psychology*, *Language Development*, *The Social Behavior of Children*, *Learning in Children*, *Children's Morals*, *Order of Birth* and *Environmental Forces* have been revised and enlarged. Eight papers have been omitted. There

are eleven papers on subjects not previously considered. Of these, Carmichael's review of the *Origin and Prenatal Growth of Behavior*, Pratt's discussion of *The Neonate*, and Gesell's treatment of *Maturation and Patterning of Behavior* are particularly extensive. New papers on more specialized topics include *Locomotor and Visual-manual Function in the First Two Years*, by Shirley, *Emotional Development* by Jones, *The Measurement of Mental Growth* by Goodenough, *Sex Differences* by Wellman, *Speech Pathology* by Travis, and *The Physiological Appetites* by Blatz. *The Gifted Child*, *The Child of Special Gifts or Special Deficiencies*, *The Child with Difficulties of Adjustment* and *The Adolescent Child* are contributed respectively by Terman and Burks, Hollingworth, Blanchard, and Hollingworth. Viewed as a whole the work is well done. Each paper is followed by an extensive bibliography and there is a complete subject and author index.

A Critique of Sublimation in Males: a Study of Forty Superior Single Men. *Genetic Psychology Monographs, Vol. XIII, No. 1.*

By W. S. Taylor. Clark University Press, Worcester, Mass. $2.00. 6½ x 9¾; 115; 1933 (paper).

In this interesting monograph the author attempts to find whether complete sexual sublimation occurs in healthy, active intellectual men in their twenties and thirties. Of the forty unmarried graduate students from whom he gathered data none had been completely successful in sublimating their sex energies into "higher forms" of activity. In 18 per cent the sexual energies found expression in nocturnal emissions only; 15 per cent found satisfaction in habitual "petting" to the extent of orgasm; 63 per cent practiced masturbation; 8 per cent had intercourse with prostitutes and 13 per cent with women other than prostitutes. In some cases more than one form of sex adjustment was practiced, so that the percentages add to more than 100 per cent. The author concludes that there is in young men an irreducible minimum of sexuality which is never transmuted or sublimated away but which requires and finds some direct outlet in every case. As to the bearing of his study on the conduct of life he concludes that

> no perfect solution for the problem of sex in the young adult male has appeared. Pending the arrival of the perfect social order that never comes, it would seem that any form of adjustment must bring with it both advantages and disadvantages. "Dreams" appear to fail many mature men who have to work hard; "spooning" is a curious make-shift, probably not lacking in social and other disadvantages; "masturbation" fails to develop the normal emotional and social life, if it does not stunt that life; "women," and "prostitutes," as "adjustments," involve several kinds of deprivation and danger, particularly to the women themselves; and early marriage, which suggests itself as an alternative, runs all the risks of youth and inexperience.
>
> Yet, under the conditions, early marriage may be the best way. The few instances that the writer has seen of married graduate students of the type studied were instances of contentment, good work, and good play . . . As for the economic factor, if it is not true that two can live as cheaply as one, it is nevertheless true that two can live as cheaply as two.

The book contains a bibliography of 134 titles and an adequate index.

Culture and Human Behavior.

By Sanford Winston. The Ronald Press Co., New York. $2.50. 5⅜ x 8; ix + 249; 1933.

In this book the author approaches the study of sociology from the cultural standpoint, which, he concludes, "is basic to an understanding of social institutions and of human behavior in general." A culture—the material instruments and the nonmaterial ways of doing and thinking which a society has developed—is based on societal adjustments to problems of human existence. The biological factors of hunger and sex, the geographical limitations of the environment, the fact that man lives a social life, pose problems to which men attempt different solutions. Those solutions which seem the most satisfactory to a group become stereotyped in its mores.

In his insistence on the functional approach Dr. Winston seems inclined to the viewpoint that all cultural traits are useful and necessary, an attitude dangerously close to Pope's "Whatever is, is right." No doubt a student of a culture should not be hasty in assuming that a trait of which he does not at first sight perceive the use is therefore useless, yet it seems equally

invalid to assume *a priori* that the trait is necessary. After all, different societies have adopted widely different means of solving a problem, so that a custom which one society regards as pernicious is found in another to work well. As organic evolution is the result of individual variation, it is likewise conceivable that societal evolution towards a more perfect adjustment of culture to needs might be facilitated by greater freedom for individual variation in behavior.

The book contains a bibliography of nine pages and an index.

PERSONS ONE AND THREE. *A Study in Multiple Personalities.*

By Shepherd I. Franz. Whittlesey House, McGraw-Hill Book Co., New York. $2.00. 5 x 7½; xv + 188; 1933.

This case history of a man picked up by the Los Angeles police and found to possess a multiple personality has the double merit of being as entertaining as a detective novel and at the same time of being a complete and objective description of an interesting psychopathic case. For this man, time began in February, 1915, during his convalescence from wounds received in action in Belgium, and by no exercise of his imagination and ingenuity could he trace his doings before that time. He was a man of more than average intelligence, and being greatly disturbed at his inability to remember his name or birthplace he had traveled widely, hoping to find some place he could recognize as home. He even attended the services of a number of different religious denominations, experimentally, in an effort to find the one in which he had been reared. Dr. Franz has made a narrative of his attempts to link up the two, or possibly three, personalities that emerged during the course of his questioning of the man, resorting to his record of questions and answers occasionally, quoting letters, and giving facsimiles of the man's handwriting at different times. Incidentally, the self-contradictions contained in the service record supplied by the British War Office are as puzzling as anything else in the book. Finally, as a result of questioning at a particularly favorable time, it was possible to enable one of the subject's personalities to recognize another, and gradually a partial cure was effected. The book is free from theorizing throughout and both psychologists and general readers will like it.

OBSERVATIONAL STUDIES OF SOCIAL BEHAVIOR. *Volume I—Social Behavior Patterns.*

By Dorothy S. Thomas, Alice M. Loomis, Ruth E. Arrington, with the assistance of Eleanor C. Isbell. Institute of Human Relations, Yale University, New Haven. $2.50. 6 x 9; xvii + 271; 1933 (paper).

A technique for observing and recording overt behavior patterns is here fully described. It differs from that of previous investigations because the authors have attempted to group the different modes of social interactions into a few well delimited categories. These are to delineate the reactions of each individual in a given length of time; the proportion of time dedicated to establish contact with other individuals, with surrounding objects, or with none and nothing. In general, behavior depends upon the nature of environment and so examples are given of modification of the technique when applied to a nursery school group, a kindergarten group, a trade school and an adult industrial group.

From this preliminary report very little can be said as to the value of these categories and their significance. Criticism based only on theoretical grounds is out of place because the authors themselves dedicate the greatest portion of this volume to seeking the many subjective and objective sources of error. This deserves unstinted praise and promises much for the conclusiveness of their findings.

THE FIRST TWO YEARS: A STUDY OF TWENTY-FIVE BABIES. *Volume III. Personality Manifestations.*

By Mary M. Shirley. University of Minnesota Press, Minneapolis. $2.50. 5⅛ x 7⅞; xi + 228; 1933.

The concluding volume of this series, earlier parts of which have been reviewed in

our pages, deals with the development of personality in the babies studied and its quantitative treatment. Observations were made on the babies at home in their natural environment. Much credit is due to the mothers who cooperated in making the records. After such thorough investigation it is found that personality differences are apparent at birth, and in one case a mother noticed a difference in the prenatal activity of twins.

> An age trend consistent with that of the group is manifested in every item of the baby's behavior . . . Each baby tends to manifest the various behavior items in approximately the same proportions from age to age. An item that is given up is replaced by another that is consistent with it . . . Each baby exhibits a characteristic pattern of personality traits that changes little with age . . . Babies manifest personality traits that are in harmony with those of their families.

There is a bibliography of 17 items and an index.

CONDITIONED RESPONSES IN CHILDREN. *A Behavioral and Quantitative Critical Review of Experimental Studies.*

By Gregory H. S. Razran. Archives of Psychology, Columbia University, New York. $1.50. 6⅜ x 9½; 120; 1933 (paper).

This monograph, inaugurating a series of reviews of experiments on human and animal conditioning, presents a systematic and comprehensive account of the work on conditioning of children, conducted by Russian and non-Russian workers. It is divided into six parts as follows: I. Experiments from Krasnogorski's Laboratory; II. Experiments from Chuchmarev's and Lenz's Laboratories; III. Experiments from Ivanov-Smolensky's Laboratory; IV. Experiments from Bekhterev's Laboratory; V. Individual Experiments, both Russian and non-Russian; VI. Summary and Conclusions. The treatment is behavioral in that cerebral theories are excluded, quantitative in that statistical measures have been computed from the data, and critical in pointing out factors uncontrolled by the experimenters. The book contains a bibliography of four pages, 19 photographs and diagrams, and 44 tables, and will be a welcome contribution to the psychologist and student of conditioning and learning, especially since some of the work included has not hitherto been available in English.

AN INTRODUCTION TO ANIMAL PSYCHOLOGY. *The Behavior of the Rat.*

By Norman L. Munn. Houghton Mifflin Co., Boston. $3.00. 5⅜ x 8; xxii + 439; 1933.

The literature on the behavior of the white rat is here extensively and systematically reviewed. The author's knowledge of the subject and his objective criticism render the whole a complete and excellent treatise necessary for the student in this field. There are reported the investigations on unlearned behavior, sensory processes, learning processes, symbolic processes. Many of the experiments are quoted at great length and the apparatus and methods used are described in detail. There is a detailed bibliography.

THE DEVELOPMENT OF LEARNING IN YOUNG CHILDREN.

By Lovisa C. Wagoner. McGraw-Hill Book Co., New York. $2.50. 5½ x 8; xiv + 322; 1933.

This book traces in an interesting fashion the development of learning in young children. Among the important points discussed are the steps by which the growing individual reaches maturity, his progress in the mastery of his own body, and the technique by which he acquires the skills and methods of response which permit him to take his place in modern society. The author, through contact with nursery school children, points out a number of their behavior problems and suggests methods of dealing with them. This book should have appeal both to the psychologist and the parent or teacher. The value of the work is enhanced by its systematic treatment of the subject as well as its extensive bibliography.

PSYCHOANALYSIS AND MEDICINE. *A Study of the Wish to Fall Ill.*

By Karin Stephen. The Macmillan Co., New York. $2.50. 5¼ x 7¾; vii + 238; 1933.

This elucidation of psychoanalysis is both penetrating and clear. In discussing the "wish to fall ill" the author gives a full summary of Freud's doctrines. This neurosis, which is often met with in medical practice, according to the author is due to repression also. The mechanism of repression is described—how the need originates, and how, when repression threatens to fail, the inner conflict is resolved by symptom formation.

Examples and illustrations similar to those given by Freud and others are used, but the author apparently draws a great deal from her own experience.

BEHIND THE DOOR OF DELUSION.

By "Inmate Ward 8." The Macmillan Co., New York. $2.00. 5¼ x 7½; xvi + 325; 1933.

A former journalist, suffering from dipsomania and for this an inmate of an insane asylum, describes briefly his environment and some of his companions, their thoughts and reactions. With studied simplicity and pathetic humor he seeks to emphasize the tragedy of their situation and thus arouse greater sympathy for their fate. Very interesting is his account of the agitation of his fellow inmates on the question of sterilization of the insane. Their expressed opinions with which he fully agrees show that they possess a clear realization of its significance and probable effects.

DIE VITALE PERSON. *Sammlung psychiatrischer und neurologischer Einzeldarstellungen Band II.*

By Ernst Braun. Georg Thieme, Leipzig. 7 marks. 6⅞ x 10¼; v + 79; 1933 (paper).

A psychiatrist considers *Homo sapiens* in his psychological, biological and clinical aspects.

DE OMNIBUS REBUS ET QUIBUSDEM ALIIS

SCIENTIFIC THEORY AND RELIGION. *The World Described by Science and Its Spiritual Interpretation.*

By Ernest W. Barnes. The Macmillan Co., New York. $4.00. 6⅛ x 9¼; xxiv + 685; 1933.

The viewpoint from which this book, a revised form of a series of Gifford Lectures, is written is that "Theology cannot be based solely on human spiritual experience; it must take account of the God of Nature revealed by science." With this end in view the Bishop of Birmingham devotes the greater part of his work to a technically competent and very well written exposition of the present-day results of physics, astronomy, biology and anthropology. Beginning with the dynamics and structure of matter, he deals with space and the rival systems of geometry which have been developed to represent it; the special and general theories of relativity; the electrical theory of matter; heat and light; the quantum theory and Röntgen rays; the solar system; the galactic universe and the great nebulae; the origin of life and the geological record; the evolution of plants and animals; Mendelism; the machinery of evolution; and man's origin and past. This part of the book will be of value to anyone who wishes a connected account of modern scientific results, whether or not he is interested in their bearing on theology.

But there remain problems, both of metaphysics and of theology, to consider. How far does science give us knowledge of a 'real' world? The author's position is that of moderate realism. Objects have a real and independent existence because they exist in the Mind of God. But God is not only a knower but a doer as well. The genetic variations which are the basis of evolution are, by the Bishop's interpretation, the result of His creative activity. Yet there is a difficulty here. Such genetic variations are non-moral. The problem of evil, which has plagued men's minds since the time of Job, still remains to be solved. Yet, although the Bishop recognizes that "all attempts to

take from God responsibility for the nature of his creatures must fail," he is firm in the conviction that a God who has created men to seek after righteousness must be a good God. (Reginald the Office Boy says it is mean to stick pins in the legs of a first-rate scientific man just because he wears gaiters.)

THE UNIVERSE OF LIGHT.
By Sir William Bragg. Macmillan Co., New York. $3.50. 5¾ x 8½; xi + 283; 1933.

Sir William Bragg's account of the properties of light ranks as one of the best pieces of popular scientific writing of the year. It is based on the Christmas Lectures delivered at the Royal Institution in 1931 and covers about the same ground as the section on radiant energy in an introductory college physics textbook but in a quite different way. Familiar observations and photographs, some of them colored, of lecture-table experiments have been used in addition to diagrams in order to demonstrate general principles instead of the use of algebraic treatment. Instead of being obliged to use water waves as an illustration of wave motion nowadays a lecturer can call upon popular experience with radio waves, with some gain in clarity. He has taken as the thread of his story the old rivalry between the corpuscular and the wave theory of light "which has been one of the most powerful contributors to the development of science." Everything has been nicely calculated to arouse the interest of an inquiring boy and to start him looking for illustrations of these self-same principles in things he can see about him, automobile headlights, the lustre of fabrics, and meteorological phenomena, for instance, and this isn't a bad result for a book to produce in anyone.

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